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INTRODUCTION.

ON THE IMPERFECTION AND IMPROVEMENT OF MEDICINE IN GENERAL—SUMMARY OF THE CONTENTS OF THE PRESENT VOLUME.

Since the year 1780 to the present time, a period during which many important discoveries and improvements have been made in the sciences that are subservient to medicine, more especially in animal and vegetable physiology, in chemistry, natural philosophy, comparative and microscopic anatomy, and the materia medica; a period, also, in which the rules for questioning nature have been often explained, enlarged, and strongly recommended by several eminent philosophers; the theory and treatment of diseases have exhibited the mortifying picture of as great vacillation and uncertainty as at any time of its history. Remarkable instances of the kind abound in the contradictory systems of theoretical and practical pathology which have succeeded each other so rapidly within the last sixty years, and in no part of medical science has this been more

evident than in regard to the doctrines and cure of some of the most common forms of disease which afflict mankind.

Not only in Great Britain, but in France, Germany, and Italy, medical writers seem to have vied with each other in gaining temporary fame by the invention of hypotheses, which have set all experience at defiance, and led to dangerous experiments on human life on a large scale. Examples of this kind will be brought forward in proof of these assertions in the second Commentary of this volume, where I treat of typhous fever.

It may be thought by some that I might have enlarged the sphere of my animadversions, and have adduced other instances of extraordinary opinions and practice which, by the greatest number of learned and respectable physicians of Europe, are considered to be disreputable to the medical profession in the present enlightened century. I might have hinted for instance at the juggleries of a species of modern necromancy, which has found advocates and professors among regularly educated physicians of this and other countries; but I should as soon have thought it worth while to examine seriously the mysteries of the Jewish Cabbala by which the truly learned Fludd, in the sixteenth century, explained physiology and the phenomena of nature, or to have attempted to account for the practice and conscientious conviction of Valentine Greatrix,

who, in the beginning of the seventeenth century, pretended to cure all diseases by the imposition of his hand.

Against credulity and empiricism reason has no power. The first is a weakness of the understanding which renders its victim deaf to argument; the second springs from a vicious propensity over which there is no control, as no punishment has as yet been invented for correcting it.

I have watched with considerable anxiety the rise, progress, and effects of the opinions and innovations in practice, which I have taken the liberty to criticise. At the moment I am writing these lines, I am in my seventy-ninth year, and, having passed the greater part of my life in the active pursuits of the medical profession, I cannot believe, now that I have retired from general practice, that the freedom of my animadversions will be attributed to false ambition or to any unworthy motive, but rather to the true one, namely, a sincere desire of rendering some service to an useful and a very difficult art.

It is wonderful how seldom medicine is fairly estimated either by the learned or unlearned classes of society. Extreme opinions concerning it are common to both. By some of its too zealous advocates it has been extravagantly eulogized as a perfect and exact science; by others, it has been held up to ridicule as one which is wholly fallacious and empiri-

cal. The eulogy and the contumely are equally incorrect and unreasonable.

Medicine is a science of observation and discovery, the deductions from which are employed for the maintenance of health, and the elucidation and cure of diseases.

Some of the facts which are collected by patient and correct observation, as well as those which are discovered by mere accident, are of so simple, clear, and unmixed a character, as to command at once a perfect unanimity of opinion concerning them. But others, (and these are unfortunately by far the most numerous,) are of so complicated, unsteady, and fleeting a nature, as to be of difficult scrutiny and to be differently seen and described by different individuals, and, consequently, they do not serve the purposes of correct reasoning.

The facts of the first class belong entirely to sciences which are merely auxiliaries to the study of diseases, such as those of anatomy, some parts of chemistry, and the natural history of drugs. Those of the second, are the usual manifestations of health and disease, the description which people give of their sensations, and almost everything relating to causes of disease, and the action of medicaments on the system.

The symptoms of disease, the order of their succession, and their apparent connexion and depend-

ence on each other, the agency of causes, of climate air, and food, are so diversified by individual constitutions or idiosyncracies, as well as by age, sex, and previous disorders, as to render them unfit for the purposes of correct comparison; and as the operation of medicines on different people is equally diversified by the same causes, exact imitation of former trials and experiments which constitute practical experience becomes equally liable to doubt and want of unanimity.

To endeavour, then, to conceal the imperfections of medicine as a science, is vain; nay more, it is injurious to its true interests, for how is it to be improved, if its defects be not pointed out and acknowledged?

Man is doomed to depend for his life and happiness in this world much more on the exercise of imperfect than of perfect sciences. The more deeply they are studied, the stronger is the conviction which they leave on the mind, that they will always remain in a state of great imperfection, though not perhaps to the same degree as at present.

Such is the case with the pretended sciences of government and legislation, of finance, of trade, commerce, and political economy.

The doctrines of these highly-favoured and nobly-rewarded sciences are as unsettled, hypothetical, and contradictory, as those of medicine; not merely because, as in medicine, many of the facts on which

they are built cannot be proved to be true, or because they are complicated, changeable in their nature, difficult to be collected or methodically arranged, but also, because the professors of such sciences are exposed to causes which tend at all times to disturb their judgment, and from which the physician is happily free, namely, strong and baneful passions arising from the lust of power, party spirit, and political hatred. Over these highly-honoured sciences medicine may boast of one advantage with which its professors as a body may console themselves in their humbler path of life. The errors of their opinions and practice can only affect the welfare of one individual at a time, whereas the crude hypotheses and vagaries of an empirical statesman, lawgiver, financier, or political economist, or of a board of trade, when carried into execution, may destroy the happiness of a whole nation at once, or, at least, seriously injure the interests of numerous classes of its inhabitants.

But to return to medicine: to the imperfection, as well as to the improvement of which, for the future, I shall confine my remarks; it may be observed, that the defective parts of pathology and therapeutics are owing to causes which have not been examined with that minute attention which they merit.

Although the history of diseases, as it is commonly called, (meaning by this expression a detailed account of their symptoms,) is arrived at great per-

fection, nevertheless, it is far from being so complete as is desirable for practical purposes, in many diseases of a very serious character.

In support of this assertion, I need only refer to some of those of the brain, spinal chord, the heart, liver, and digestive organs, the true distinctive symptoms of which are by no means agreed on. It is to be hoped that patient and attentive observation, combined with such discoveries, as minute *post mortem* examinations of the bodies of patients disclose, may throw light on the art not merely of recognising such disorders, but of elucidating their nature. We are encouraged in such an expectation when we reflect on the many truly important additions that have been made to our knowledge of very complicated and obscure diseases by hospital physicians, especially by those of France, who, to extensive opportunities of collecting the symptoms, have also enjoyed, without restraint, the liberty of inspecting the bodies of the dead. The semiotic observations and dissections of Laennec, Andral, Broussai, Pinel, Esquirol, Georget, Foville, and many others, are treasures of useful information. I do not say that the deductions which they have drawn from their own discoveries are correct. In the Second Commentary of this volume, I have been under the necessity of dissenting from some which are truly important in relation to fevers; but such a difference of opinion rather enhances than diminishes the value

of the facts themselves, because without such facts, a critical examination of important doctrinal questions would not have taken place.

A very great obstacle to rightly understanding the nature of diseases, has arisen in all times and countries from too great and premature a desire of explaining them. Many men of genius are, from the very condition of their mind, incapable of a patient examination of every symptom of a disease. They are impressed with a few facts only, but which appear to them to be sufficient for arriving at general conclusions, which explain the whole disorder. These they have dignified by the name of *proximate* causes. Their influence on practical medicine has been great in some cases.

I do not deny that some of the hypotheses have been useful for a time and in a limited degree, according to the sagacity employed in their construction; but others, and especially a few of recent date, have been eminently hurtful, merely because they have been hastily deduced from too limited a number of facts, to the exclusion of others which were of equal importance for arriving at fair conclusions in pathological investigations.

One of the most remarkable instances of this kind has been reserved for modern times; I allude to the doctrine, that all fevers are symptomatic, that, in fact, there is no such thing as a truly idiopathic fever. This notion, which has led to very dangerous

innovations in practice, was deduced from an anatomical fact very erroneously interpreted. Every other fact which throws light on fever was passed over, as if irrelevant to the establishing this sweeping proposition. A more circumstantial account of this, and of its effects on the treatment of fevers, will be found in the second Commentary.

As all sciences, whether perfect or imperfect, consist in generalisations or laws which are deduced from appearances or facts, the importance of correct observation in collecting them will be readily admitted. But the difficulties to be overcome in medicine by the employment of this faculty, even when it is sharpened by zeal and great attention, and also when it is restrained by caution, are greater than are commonly imagined or well understood.

Were all the facts on which we are forced to reason in medicine evident to the senses, we might imagine that diligent and patient inquiry would be sufficient to collect and compare them; but as many are in themselves naturally obscure and complicated, and many others are obtained at second hand from the accounts which patients give us of their sensations, and in which narratives there is seldom a strict adherence to truth, the evidence on which our judgments are formed are often perplexing and contradictory.

Suppose a theory of fever to be deduced from such facts as we commonly collect by observation

alone; for instance, from evident exciting causes, such as contagion, or exposure to a cold and moist atmosphere after severe and long-continued exercise, or from a cold damp habitation and want of food; and also from the first symptoms of altered health, and their apparent relation with those that follow in regular succession; and, moreover, if we also collect and add the phenomena of spontaneous cures, commonly called efforts of nature, and the effects of medicines on the symptoms, and the appearances in the bodies of those who die of the disease; such an hypothesis might boast of being constructed agreeably to the approved rules of induction. But it does not follow that such a theory would be perfectly satisfactory, either as a true explanation of the disease, or as a guide to good practice, for reasons which I shall try to explain. But I first ask if any of the modern hypotheses of fevers can boast of having had a due regard even to all of these more evident and essential data, in the invention of what is called the *proximate causes* of disease?

It may perhaps startle many when I declare it to be my opinion that there is no such thing in nature as a proximate cause of any *complicated* disease, such as typhus, in which every function of the body is more or less deranged!

The only true cause of the complaint is that which is foolishly called *a remote one*, being that which first disturbs the relationship between vitality

and the physical forces which exert their power on our solids and fluids, and on a due balance between which health depends. In fevers, many of the earliest symptoms appear synchronously, and are by no means consequences of each other: they spring simultaneously from the first influence of the exciting cause. Afterwards, indeed, there follows a succession of causes and consequences, but none of them is of so influential a character as to merit the appellation of a proximate cause of the disease.

At the end of the second Commentary I have attempted to illustrate this idea more fully, and to show how a rational explanation of fever arises from an attentive analysis of the symptoms, and by a reference to analogy, to the efforts of nature, and to the facts to be derived from empirical practice and the inspection of the bodies of those who die of the disorder; to which are added the important facts which an improved state of physiology supplies. I do not treat the subject of typhus systematically; I take it for granted that the history of its symptoms is known, and therefore I begin the Commentary at once with what is practical, giving an account of the general treatment as it was exercised in London about fifty years ago, and after describing and animadverting on the many singular and dangerous innovations in doctrine and practice which followed each other since that time, I offer such an explanation of the malady as I think elucidates the

principal facts, and leads to safe rules of practice, seizing the opportunity of embodying all I know on the subject.

To account for the first Commentary in this volume, I must premise a few observations.

The most remarkable phenomena in fevers, independently of the affection of the brain, being the disturbance in the functions of the heart and arteries, it appeared to me to be necessary to discuss the merits of two very opposite doctrines concerning them, namely, that of Haller, which is maintained to this day by Dr. Wilson Philip, and which he pretends to have demonstrated by experiments, an account of which is to be found in many of the late volumes of the Philosophical Transactions, as well as in other works which he has published apart. The other doctrine is that of the late Dr. Thomas Young, which is entirely opposed to that of Dr. W. Philip, and the demonstration of which, although little noticed, made the subject of the Croonian lecture for the year 1808. (See Phil. Trans. for 1809.) So slow is the progress of truth in some cases. The motives for taking a part in this discussion arise from its importance to many points of practice; but as a full account of them is detailed in the commentary itself, I merely refer to it here as an apology for its appearance.

The third Commentary is of a miscellaneous character, though it relates wholly to the manifestations

and nature of mental derangement. It must be known to some that so long ago as the year 1790, I published a work on this subject, and it might therefore be expected that after a lapse of so many years experience might have furnished me with many important observations. But when it is considered that the moral as well as physical treatment of the insane is almost exclusively confined to the skill and talents of physicians of hospitals, asylums, and private retreats of lunatics, and that at no period of my life I had any of these under my care, it ought not to appear surprising that my experience of these diseases, as far as daily observation is concerned, has been of a limited description.

It would perhaps have been prudent on my part if I had refrained altogether from offering opinions on questions which belong to the subject of insanity.

But some of the most important of these appear to me to be deserving of more minute examination than has been bestowed on them, notwithstanding the number and bulk of the essays and works that have been published on such subjects. New opinions, or even a variety of sentiment on such matters must excite new inquiry and discussions, which, if conducted as objects of science ought to be, cannot fail to be beneficial.

I have confined myself in the present volume to a very few of these, such as the formation of a correct and practically useful definition of insanity, its true

cerebral character, more especially that of mania, a few observations on its remedial treatment, and, lastly, on the attempts which are being made to impute certain vices of character as well as crimes to insanity, with a view of palliating them, and of condemning the severity of the law. On such subjects I conceive that every well-educated physician is as competent to form a correct judgment as those who devote their whole time and talent to the insane.

Lastly, I have added extracts from the reports of the General Hospital of Hamburgh on a disputed point of practice, which, I confess, belongs more to surgery than to medicine, and which, therefore, seems to require an apology, although it is well known that surgeons are not particularly scrupulous how they encroach on the province of physicians!

In that part of the second Commentary which treats of the exciting causes of typhoid fevers, I found it impossible to avoid an inquiry into the agency of contagion; and this naturally led me to notice the truly ingenious hypothesis of Dr. J. Liebig concerning the manner in which certain animal poisons reproduce and multiply themselves in the human fluids. I have endeavoured to give a concise and clear account of his doctrine, which I think is supported by many phenomena of disease. But his theory left unexplained a well known fact, namely, that it was the property of some animal poisons to excite febrile commotions, which, although specifi-

cally distinct from each other, (as in the case of small-pox, measles, and plague,) yet agree in one general character, namely, the short and limited nature of such diseases; while others, such as syphilis and the yaws, produce chronic diseases which are of indefinite duration.

On this subject I have proposed an explanation, which consists in this,—that in all contagious or infectious diseases which excite specific fevers, the supply of fresh elements for the purposes of chemical metamorphoses in the blood is cut off by the very nature of fever itself, the desire for food being either suspended or abolished; whereas, in infectious diseases which progressively increase in force for an indefinite time there is no natural hindrance to the supply of fresh materials for the virus to act on. Now in conformity with this opinion it ought to follow that syphilis can be cured by rigid abstinence and such evacuating remedies as may be safely given to patients who are nearly deprived of sustenance; nay, what is more, the Hunger cure ought, if properly conducted, to prove a more perfect security against secondary symptoms than a mercurial course of treatment, because it is a certain method of destroying the very last traces of virus. In support of this doctrine I have brought forward a body of evidence which I believe to be imperfectly known in this country.

I do not offer any apology for having confined my

remarks to so few diseases as are treated of in this volume: for, however desirable I may deem it to be, that every part of doctrinal and practical medicine should be reviewed from time to time, while physiology, and especially chemistry, is throwing new light on the functions of the human body, and on the causes and symptoms of diseases, yet I feel that I have passed that period of life when the exertions of thought are equal to such an extensive and difficult task. All I hope for is, that the method I have employed may prove useful to the student, and engage others to enlarge and improve it.

A. Crichton.

June, 1842.

COMMENTARIES.

COMMENTARY FIRST.

ON THE FUNCTIONS OF THE HEART AND ARTERIES IN HEALTH AND DISEASE.

Importance of the subject in a practical, as well as a physiological point of view—Haller's doctrine of the muscular action of arteries demonstrated to be erroneous by Dr. Thomas Young in his Croonean Lecture for 1809—notwithstanding which, it is still maintained—Justice has not been done to him by any modern physiologist—Difficulty of following his chain of arguments from the depth of his mathematical information—Attempt to give a popular version of his dynamic theory of the circulation—Arteries are perfectly elastic tubes—They have no action but that which is derived from their elasticity—If they performed the function of muscular contraction and relaxation, it would prove to be a hindrance to the circulation—Calculations by Hales and Keil of the loss of momentum to the impulse on the blood by the heart's action from natural causes—Application of the result to practical pathology—Quickness of pulse is not a proof of quick circulation—Reasons for believing the contrary opinion—In typhous fever the progressive motion of the blood is slower than in health—True nature of the pulse, with some practical deductions—Under what condition of the pulse the circulation may be presumed to be quicker than in health—Danger of violent muscular exertions after a full meal—First symptoms of organic mischief from this cause—Inquiry continued—Discoveries of recent anatomists concerning the true character of the middle coat of arteries—Examination of Dr. Wilson Philip's experiments, and refutation of his hypothesis.

THE subject of this commentary is one which involves questions of great interest, and of very extensive applica-

tion, not only in physiology, but also in practical pathology ; for, in fact, the explanation of the phenomena of every function of the body, as far as its healthy state is connected with the distribution of the blood, must depend on a correct knowledge of the forces which impel that fluid through every part of our frame ; and, for the same reason, the treatment of diseases, in which a disordered circulation forms an essential part, must also require the same kind of accurate information.

It is lamentable to think, that at the distance of above two centuries from the time of Harvey's great discovery of the true course of the circulation of the blood, there still should be much difference of opinion among modern physiologists of equal reputation, concerning the action of arteries and capillary vessels; for, although the force, on which all living organic movements depend, is hitherto a perfectly hidden mystery to us, yet it seems almost incredible, that any dispute should exist concerning the true character of arterial action, seeing, that minute anatomical investigation, ingeniously-contrived experiments, chemical analysis, and correct mathematical reasoning, are sufficient to settle the question.

Until the publication of the Croonian Lecture of the late Dr. Thomas Young in the Philosophical Transactions for 1809, it was an almost generally received opinion, that the blood, after its expulsion from the heart, was propelled in the arteries, and especially in the capillaries, by a regular succession of muscular contractions and relaxations of these vessels, which were similar in kind, though not in degree, to the systole and diastole of the ventricles and auricles of the heart.

This doctrine, the author of which was the celebrated Haller, was founded partly on an anatomical mistake; namely, that the middle coat of arteries was of muscular structure; an opinion, which more correct research and

chemical analysis, have now proved to be erroneous, and partly, on a singular error in reasoning, even if we admit the existence of a muscular coat in the vessels.

The error in reasoning was first clearly pointed out by Dr. Young, to whose merits in this part of physiology, justice does not appear to me to have been done. His profound knowledge in the higher departments of mathematical learning led him, as soon as he turned his attention to the subject, to discover that it was impossible to explain the progressive motion of the blood from the ventricles of the heart to its auricles, if it were assumed that the arteries performed muscular contractions and relaxations like the heart itself. And the detection of this false hypothesis was the more remarkable on the part of Dr. Young, because, at the time he wrote, anatomists, and he himself also, believed that the arteries were endowed with a muscular tunic. We shall see how he tried to reconcile the belief in this anatomical error with the conclusions to which his calculations and better reason led him.

The desire of doing justice to the merits of one whom I had the honour of counting among the number of my earliest pupils when I taught practical medicine in London, and the conviction that many obscure parts of pathology are best elucidated by his opinions, (modified, however, by more recent researches,) have urged me to make it the subject of my first commentary: and I deem this the more essential, because I find, nearly twenty-five years afterwards, an eminent physician and most ingenious and zealous experimental physiologist, Dr. Wilson Philip, filling several volumes of the Transactions of the Royal Society with essays in favour of the old, and what appears to me, untenable doctrine.

If it were only a subject of physiological curiosity to ascertain which opinion was the truest, or, shall I say,

the most probable, I do not think that, as an old practical physician, I should be justified in taking part in the controversy; but, looking up to physiological science as I do, as the only ground of hope for the solid advancement of medicine, I hope to be excused on that ground.

It must be admitted that, it is by no means easy to follow Dr. Young's arguments. To him the more refined departments of mechanical philosophy were familiar, and, in writing a paper for so learned a body of men as the Royal Society, he did not, I presume, think it worth while to give a popular account of his doctrine, but rather studied to demonstrate its truth by mathematical calculations.

It is not my intention to introduce these into this essay, but I shall lay before the reader, in as clear and plain language as I can, the chief data on which he reasons, together with his conclusions; for, in fact, to the practical physician and surgeon, these are all that need to be remembered. To any one who suspects me of having misrepresented his opinions, or who imagines his arguments to be defective, I must refer to the Croonian Lecture itself.

Admitting for a moment that the function of the arteries and the capillaries in propelling the blood, consists in alternate contractions and relaxations, a slight calculation was sufficient to show Dr. Young that the motion of the blood from the right to the left side of the heart, could not be promoted by such action; for the first, or systole, would oppose the progressive motion, by a repetition of mechanical hindrances; and the second, or muscular dilatation, (diastole,) would produce a similar effect, by repeatedly taking off the pressure. Let us suppose the left ventricle of the heart injects into the aorta, at each contraction, from an ounce and a half to

two ounces of blood, and that this quantity occupies any given space of that vessel; it is clear that such a portion of the artery would represent the length of such parts of it, as would be thrown, in succession, into alternate contractions and relaxations agreeably to the old hypothesis, or to that which is still maintained by Dr. Wilson Philip.

Every ramification or branch of the aortic system must be supposed to act in a similar manner; the length of the portions contracting and relaxing, necessarily varying with their diameters, and consequently, with the length of the vessel requisite to receive the blood so propelled from one of a greater capacity.

It must also be admitted, according to this doctrine, that those parts of an artery which are immediately contiguous to the portion which is in a state of contraction, must of necessity be in an opposite state or in one of relaxation; inasmuch as they must just have finished their systole. This observation is equally applicable to the part which immediately precedes, as to that which succeeds to the portion which is in contraction. But as the pressure on the blood is withdrawn during dilatation or diastole, it must follow, if the doctrine be true, that when a portion only of an artery contracts, part of the blood thus compressed, would be forced backward as well as forward; for the arteries not being supplied with valves like the veins, there is nothing to oppose such a retrograde motion; because we must suppose, agreeably to this hypothesis, that the portion of artery which is contiguous to that which has just been filled with blood, and which is about to contract, is empty at that second of time. There is, therefore, no reason why it should not receive a portion of blood in this manner as well as from that which is nearer the heart; and if we suppose only one tenth part of the blood which is compressed, to be forced

backward, (and there is no reason why it should not be greater,) an increasing retardation of the progressive motion of the blood would be inevitable.

As to the diastole of an artery, or, rather of alternate portions of it, which is assumed by the hypothesis, it either must be a spontaneous action, like the diastole of the ventricles of the heart, when that organ, while yet alive, has been cut out of the body, and consequently not stimulated by blood; or else, the dilatation is a forced one, arising from the injection of blood from the portion of artery immediately preceding it when in its state of systole; but, whichever of these imaginary conditions is believed to be true, the conclusion is inevitable, that the diastole must retard the current of the blood. For if it be spontaneous, all pressure is nearly removed; and if it arises from forcible dilatation, the friction of so thick a fluid as blood would produce retardation.

Dr. Young, who believed in the existence of a muscular coat of arteries, was so convinced of the impossibility of explaining the phenomena of the circulation on the principle of alternate muscular contractions and dilatations of these vessels, that we find him inventing an hypothesis to reconcile its supposed structure with his reasoning; and he concludes that the sole purpose of the muscular coat is to increase the elasticity of the artery: for, in fact, the great object of his very learned lecture is to prove that the propulsion of the blood from the left to the right side of the heart, is entirely owing to the impetus it receives by the contraction of the ventricles, and to an equal and forcible pressure excited by the arteries, which he justly considers as perfectly elastic tubes. This pressure, according to Dr. Young, is proportionate to the previous distension of the arteries; for such is the law which regulates the pressure of an elastic tube on its contents; and it must be evident that this

again depends not merely on the structural and healthy tonicity of the artery itself, but on the strength and health of the heart which injects the fluid into it. To the calculation of such forces, as well as those of the obstacles which arise from the ramification of the arteries, and from the friction of so thick a fluid as the blood, the greater part of Dr. Young's ingenious paper is dedicated.

In calculating the force which the heart is obliged to exert in order to overcome the resistance that is opposed to it by the ramifications of the arteries, and especially of the capillaries, where, according to Young, the chief resistance exists, he adopts as the data of his reasoning, the results of some beautiful experiments of Dr. Hales, an account of which is to be read in the Hæmastatics of that author; and for data on which he builds his conclusions concerning the resistance of the blood itself as a dense fluid, he refers to various experiments and theorems of his own, which are published in the Philosophical Transactions, on the transmission of fluids of different degrees of density through tubes of glass and other elastic substances. Dr. Young argues from the experiments of Hales, that the blood in the human arteries is subjected to a pressure from the action of the heart, which is measured by a column of seven and a half *feet*, whereas in the veins, the column does not rise to more than six *inches*. About this time, Dr. Keil published an account of many valuable observations and experiments, with the view of ascertaining the resistance to the momentum communicated to the blood by the heart. His mode of proceeding consisted in comparing the quantity of blood which flowed in a given time, from the divided crural artery and crural vein of a living dog. The quantity obtained from the artery, when compared with that from the vein (the space of time allotted for

each experiment being the same) was as seven and a half to three. According to the experiment of Hales, the loss of momentum was equal to the pressure of a column of blood of seven feet. Here, then, let us pause for a moment, and consider the probable consequences of this great loss of momentum.

When the blood has passed through the capillaries, and has sustained this loss of pressure, the very slightest reflection must show us, that if any agent, whether moral or physical, whether a depressing passion or a narcotic and deleterious drug, acts on the heart in such a manner as suddenly to weaken its power, while the quantity of blood is nearly the same as in health, the effects must be either sudden death, or a very alarming state of weakness and lipothymia; for it is evident, that the aërification of the blood in the lungs, on which life depends, can no longer take place in the required time. Hence, sudden terror and sudden grief have at times produced death or an alarming fainting fit, not so much by their direct influence on cerebral and nervous energy, as by overcoming the vital force generally; and thus weakening the contractions of the ventricles of the heart, so that the auricles do not receive their supply in time.

The danger which arises from the administering in full doses powerfully sedative medicines, such as prussic acid, digitalis purpurea, colchicum, and belladonna, and the like, is not, I am convinced, sufficiently reflected on by some practical physicians and surgeons, who probably think it adds to their reputation, to be called bold and decided practitioners; rash and injudicious practitioners would be the proper appellation for all such men. The risk of suddenly paralysing the brain, and consequently the heart, by large doses of prussic acid, digitalis, and the like, is well known; and it may be confidently asserted that the most extensive experience

with these powerful medicines and with individual constitutions, never can enable us to predict with certainty the resistance which the vitality of any patient is capable of making to such disturbing causes. It is for this reason that another kind of practice, which is the offspring of apparent caution and prudence, becomes almost as dangerous in regard to the administration of prussic acid, or any other powerful sedative, as the exhibition of too large a dose, I mean that of ordering a gradual but daily increase of the quantity. A patient may be apparently deriving benefit under such a plan, when unexpectedly the addition of a single grain or a minim stops the circulation for ever.

The hardihood with which these medicaments is sometimes given cannot be justified. They are dangerous examples to follow, especially by the inexperienced; besides, it is a false notion to suppose that a quick pulse, for which such remedies are prescribed, is a sure indication of quick circulation. The pulse may beat one hundred and thirty times per minute, and yet the progressive motion of the blood from ventricle to auricle may be slower than in health. No doubt it will appear paradoxical to such as believe that the pulsation of an artery is owing to its independent muscular action, that in proportion as the pulse beats oftener, the progressive motion of the blood is probably proportionally slower. But let us reason dispassionately and without prejudice on this curious question, and as one which demands new examination.

Suppose the left ventricle to eject about an ounce and a half of blood (J. Müller says two ounces) at each contraction, and that there are seventy-five such contractions, and consequently pulsations, per minute, it then follows by the calculations of Dr. Young, that the mean velocity of the blood in the aorta, is eight and a half inches per

second of time; the velocity in each of the succeeding segments of the artery must of course be smaller, in proportion as the joint areas of the branches are larger than the area of the aorta or chief trunk. It must also be recollected that the aorta is already distended with blood, though not fully so, when the ventricle contracts. When this happens it suffers an additional though only a minute dilatation. It is this sudden swelling of the artery, which produces the pulsation, and which is transmitted with such celerity to the remotest capillary. But this is the mere motion of a wave or undulation of the blood. It is not a succession of muscular action in the artery. This wave, according to Young's accurate calculation, travels at the rate of *sixteen feet* per second, whereas the blood, even in the aorta, does not move forward at a greater rate than *eight inches* per second. Deducting this from the motion of the wave, it leaves about fifteen and a half feet per second for the real motion of the pulse; the pulse, therefore, being an indication of a mere wave or undulation of the fluid, has no more relationship to its actual progressive motion, than the waves which are formed by throwing stones in a river have with its actual current. It is as distinct as the undulations of sound are from the currents of air or of the wind; and consequently, the rate of pulsation cannot be an index of the progressive motion of the whole mass of blood.

When the heart is debilitated, either suddenly or gradually, no matter whether from moral causes, such as terror, or from the influence of powerful miasmata, it has not a force sufficient to empty its cavities completely at each contraction; but the progressive motion of the blood depends, not only on the force with which the heart acts, but also on the quantity of blood ejected by the ventricles. In adynamic fevers they probably do not expel half an ounce of blood at each systole; and as the

impetus with which this is projected into the pulmonary artery and aorta, is diminished in proportion to the loss of muscular power, we may conjecture the loss of impetus to the whole contents of the arterial system. We must also recollect, that the vital elasticity of arteries is weakened by whatever diminishes nervous or cerebral energy. In such a case, every cause, without exception, or, in other words, all the forces on which circulation depends are diminished, and a retardation of the general current of blood ought naturally to result from such condition. Towards the end of fatal typhous fevers, when the hands and feet become livid and cold, and petechiæ and vibices appear, from carbonized blood stagnating in the capillary vessels, when the features shrink, and everything denotes congestion of blood in the larger arteries, the pulse increases in quickness, as the vital powers begin to cease. Can it be believed, that in this state of the body, the general current of blood is quick, or at all proportioned to the quickness of the pulse? The discoveries of modern physiologists throw much light on the real state of the circulation in cases of great debility, such as may be supposed to exist in the advanced stages of fatal typhus. If the exhaustion is so great as to diminish in a remarkable degree the force of the heart's action, the red particles are observed, by means of a microscope, to have merely a periodic motion, and to remain stationary in the intervals of pulsation; and if the debility of the animal is extreme, the red particles are even seen to recede somewhat after each impulse.

These are the observations of Wedemeyer, which are confirmed by J. Müller, who repeated the experiments. They seem clearly to prove, that in cases of great debility the natural function of the capillary arteries retards the circulation, instead of promoting it; that is, their con-

tractility sends the blood backwards as well as forward, the pressure from the heart being so weak; and it also proves that in such cases the circulation depends nearly altogether on the remaining force of the heart. If, as some still foolishly imagine, the capillary arteries were endowed with an independent action, which assisted the heart in propelling the blood into the veins, this would be the time when such assistance was most wanted, but unfortunately, the contrary is the case.

It is only in diseases in which the pulse is full and strong, as well as quick, that we have any right to conclude that the general circulation of the blood is more rapid than in health; and this agrees with the results of practice, for it is only in such cases that colchicum, digitalis, or prussic acid, &c., can, or, at all events, ought to be administered in maximum doses, and even in such cases, for instance, in very acute rheumatism, in the fever and inflammation of gout, in mania ferox, the practice is often hazardous; for, although the violence of the symptoms are generally allayed by it, the secondary effects in some are alarming. Hospital physicians, who are in the habit of giving large doses of such medicaments, often with the very best success, are tempted to follow the same practice with the wealthier classes; but when they do so, they may commit a great practical error; for, although the class of labourers of all kinds are but indifferently fed and much worked, they are less injured by evacuating and sedative medicines, than the richer classes of society, who are daily overstimulated by excess of food, stimulating drinks, hot apartments, and what are called the comforts of life. The following appear to be fair conclusions concerning the pulse, as deduced from the preceding doctrine, and the facts on which it is founded.

1st. The pulse, as perceptible to the sense of touch,

depends on the action of the left ventricle of the heart, and not on an action of the arteries.

2nd. It is a good index of the force with which the blood is ejected from the ventricle, and consequently, a measure of the muscular force of the heart itself; due regard being had to the size and support of the artery which yields the sensation, and to the quantity of matter which lies between it and the finger.

3rd. The volume of the pulse, which is perceived by a delicate and long-exercised sense of touch, measures the comparative distention of an artery; and consequently, is an index of the quantity of blood which is ejected by the left ventricle at each contraction.

4th. The quickness of a pulsation, by which I mean, the short duration of the impression made on the finger, indicates the celerity with which the distending wave is propelled along the artery, but it is by no means a measure of the blood's velocity of motion.

5th. The quickness with which pulsations succeed each other, certainly measures the quickness with which the systoles of the heart succeed each other, but it is not a measure of the quantity of blood which is ejected at each contraction. In general, the quicker the pulse, the smaller is the quantity of blood ejected at each systole.

The sensation which is communicated to the finger when we compress an artery, must not be confounded with that which is owing to its dilatation; because by compressing the artery, we create a new mechanical obstacle to the current of the blood; but it is an useful practice in order to have a more correct measure of the force or impetus communicated by the heart; when an artery throbs, as is commonly said, it merely designates a greater and more frequent dilatation than usual; it is a sign of the heart's action just like the strong pulsa-

tion of an aneurismal sack, which, having lost its vital contractility, pulsates only in consequence of the blood injected into it.

When we reflect on the perfect elasticity of arteries, we can easily comprehend why the pulse becomes small and contracted in every affection and disease in which the heart is enfeebled. Every physical or moral agent which depresses the muscular force of that organ, and which, consequently, determines the ejection of a smaller quantity of blood during its systole, must of necessity create this effect. It is not the result of spasmodic constriction as formerly believed; but, although the affection of the heart, accounts to a very great extent for the phenomena, yet I will not take it upon me to assert that the feebleness of the pulse is solely owing to the feebleness of the heart's action. The vitality of the arteries, though, for the wisest purposes, not easily affected by nervous influence, is yet not wholly independent of it; and it may, therefore, happen, that very powerfully depressing passions, large doses of sedative remedies, and contagion, may, through the medium of the brain and nerves, weaken the vital contractility of the arteries, as well as the vis insita of the heart. In such a case, the effects must be very different, according to the quantity of blood in the body at the time of the affection.

The time that is required for completing the entire current of blood from any cavity of the heart until its return to the same, is not agreed on among experimental physiologists, although much pains have been taken by many eminent men to arrive at a correct conclusion concerning it.

Since the rate of the blood's progressive motion is the effect of the impetus it receives from the heart, and of the continued pressure of the elastic tubes through which it

flows, we can at once understand, how it ought to vary, not only at different periods of life, but even at different hours and moments of the day; and, in fine, according to the health, strength, and excitement of the individual.

In attempting to arrive at a knowledge of the fact by arithmetical calculation, that is, by multiplying the quantity of blood which is ejected by the left ventricle, with the number of its contractions per minute, it is evident that we ought first to know with accuracy what the exact quantity of blood is, which belongs to the individual, or, at all events, the average quantity which belongs to well-formed healthy adults. On this subject there is a great difference of opinion; some stating it to be forty pounds avoidupois weight, while others scarcely admit it to be much more than the half of that quantity. Wrisberg, who estimates the whole quantity at thirty pounds, informs us that a woman, who died of fatal hæmorrhage, lost twenty-six pounds of blood, and that from a young full-blooded woman, who was beheaded, twenty-five pounds were collected. As the solution of this question has little influence on practice, I leave it in the uncertainty in which I find it.

In order to ascertain the amount of resistance to the impulse of the heart, which arises from the friction of so thick a fluid as the blood in capillary arteries, the diameter of which does not exceed the hundredth part of an inch, Dr. Young had recourse to numerous experiments he had previously made on the transmission of fluids of different degrees of density through minute tubes of glass of various diameters; for it is evident that the experiments could not be on blood itself, owing to its tendency to coagulation. The pressure employed was equal to a column of twenty inches. The resistance to milk in a tube, the diameter of which was one-fortieth of an inch, was three times as great as that of water; a solu-

tion of sugar, in five times its weight of water, produced twice as much resistance as water itself; and a solution of the same substance, in twice its weight of water, produced nearly four times as much resistance as water alone. And in a narrower tube, the bore of which was oval, and the section of which gave one hundred and seventy seconds for its mean diameter, the weaker solution of sugar showed a resistance five times as great as that of water. From these and a number of other experiments, which must be read with great attention, he justly concluded that the greatest resistance to the propelling impetus of the heart is in the extreme capillary arteries; thereby confirming the experiments of Drs. Hales and Keil already mentioned.

Of the Effects of Exercise in connexion with the foregoing conclusions.

When we reflect that more than half of the capillaries, smaller arteries, and veins of the body, are forcibly compressed by the muscles during violent exercise, we may form a comparative estimate of the efforts which the heart must make under such circumstances, to circulate the blood. Owing to its quick return to the heart, by the compression of the veins, and the obstacle to its progressive motion in the arteries, from the compression of the capillaries, which form a net-work round every fibre of every muscle, the heart becomes loaded and distended; and consequently, under very violent exercise, the danger of disorganization is great; which, indeed, daily experience teaches us. This danger is often increased by other circumstances. If, for instance, soon after a full meal, and the drinking copiously of any fluid, and consequently, when the stomach is distended, and fresh additions of chyle are pouring in abundance into the

general mass of the blood by the thoracic duct, violent bodily exercise, or great muscular exertions of any kind, be taken, as, for instance, running up a hill, using gymnastic exertions, fencing, dancing violently, vaulting or leaping, all of which exercises are often thoughtlessly employed at such a time, the impediment to free circulation becomes necessarily so great, as frequently to lay the foundation of incurable disorganization of the ventricles of the heart, or of the aorta and pulmonary arteries.

To such a cause, may be traced many cases of aortic aneurism, and preternatural enlargement of the left ventricle. Among the agricultural labourers, and some tradesmen, and the poorer classes of society in general, to whom, since my retirement from public life, I have dedicated much of my time, aneurism of the aorta and preternatural dilatation of the left ventricle of the heart, are not uncommon diseases. Almost every case of the kind which I have met with, I have traced to the cause mentioned, namely, great muscular efforts, or to concussion from a fall or blow soon after taking food.

There is another cause of these organic lesions to which people of all classes too often expose themselves from irreflection, and which in very weak people, and also in those in whom the powers of life begin to be naturally impaired from age, is a common one both of aneurism and enlargement of the heart; I allude to the daily habit of straining violently, in order to exonerate the intestines. The retention of the breath, and consequent pressure on the great thoracic blood-vessels by the inflated lungs, the forcible depression of the diaphragm in the same act, and consequent pressure on the abdominal portions of the aorta and vena cava, together with the compression of whole systems of capillaries, not only overload the

heart with blood, but the brain also; so that sudden death from apoplexy often follows this effort.

When aneurism of the aorta, or of the pulmonary artery, or preternatural dilatation, or hypertrophia of the ventricles, arise from such causes, they often commence with slight inflammatory symptoms, the diagnosis of which is obscure and difficult. The pulse is quicker and smaller than usual; it is slightly irregular; the patient is obliged to sigh frequently, but he seldom makes any complaint, except of a dull pain in the region of the heart, and more or less oppression and general weakness. As the functions of the brain and digestive organs do not sympathize with the local affection, the patient seldom believes himself to be seriously ill. Such cases are often detected by old practical physicians, but to the young and inexperienced, they are too frequently stumbling-blocks. I have known them to have been mistaken for hypochondriasis, liver and stomachic complaints, and treated accordingly. Such errors of judgment do not always arise from want of experience, but from a careless and hasty manner, or unpardonable negligence in questioning the patient as to the causes of his indisposition. In all cases of fixed pain, or even of uneasiness at the region of the heart, however slight, if accompanied by involuntary sighing and more or less oppression, especially when these symptoms can be traced either to bodily exertion, or exposure to cold, or to damp and cold, as their cause, it is a safe and generally useful practice to take away blood both from the arm, and by leeches also; to restrict the patient to the lowest diet, and frequently to apply large cataplasms of belladonna to the chest. It is well known that weak and debilitated people are as subject to aneurism of the aorta and preternatural distention of the ventricles of the heart, from the causes mentioned,

as those who are strong and muscular. Indeed, I should say, they are more disposed to such organic affections from imprudent exertions, especially after a full meal. In these cases it frequently happens that the preliminary inflammation, as I have already stated, does not excite enough of pain to awaken the patient's attention. This absence of pain, which is a constant feature of inflammation of the substance of the heart, even in its most acute forms, is not easily accounted for, by saying that that organ is very scantily supplied with nerves; for small, as they undoubtedly are, they are sufficient to convey to the heart the slightest emotions of mind. This is one of those singular adaptations which is not to be accounted for by any physiological fact. The human heart is agitated by almost every mental emotion or conscientious feeling; which is a clear proof of nervous connexion between it and the brain, and yet it seems to be insensible to bodily pain. These are divine decrees which we do not pretend to explain.

There are other animadversions, both of a doctrinal and practical kind, relative to exercise, as connected with the maintenance of health and prolongation of life, which I wish to submit to the opinion of others; but I feel that if I entered on their consideration at present, it would interrupt, for too long a time, the prosecution of the inquiry into the nature of the dynamic forces which propel the blood. Therefore I shall add them at the end of the commentary in an appendix, and proceed with the main subject.

Inquiry continued.

Independently of the profound calculations and convincing arguments which are brought forward by Dr.

Young, against the hypothesis of an independent muscular action of the arteries, the conclusion he arrives at, receives confirmation from many recent anatomical observations of Hildebrant, Bichat, Döllinger, Rudolphi, Hodgkin, and Bright. The two latter eminent practical physicians and physiologists, concur in asserting, that the middle coat of arteries, when examined with a good microscope, exhibits a different structure from that of muscles, inasmuch as the transverse striæ, which belong to these last bodies, are totally wanting in the middle tunic of arteries; and Döllinger has shown that the capillaries of the middle tunic of arteries are distributed throughout its structure in an arborescent form, while in muscles, they form a network, which surrounds each fibre. This, in itself, is a strong argument; but the analysis of muscular fibre, and of the middle coat of arteries by Berzelius, settles the matter still more completely; for in the tunic of arteries he could not detect a portion of *fibrine;* which is so abundant and essential an ingredient in muscles. Water abounds in muscular fibres as well as fibrine; whereas the middle tunic of arteries is comparatively dry. Muscular fibre, like fibrine, is soluble in acetic acid, the arterial coat is not so; and, on the other hand, it is easily dissolved in mineral acids, while that of muscles is very sparingly so. The solutions of arterial fibre do not exhibit any precipitate by means of ferrocyanites, nor by alcalies, which however, would, happen did they contain fibrine.

The facts and arguments which I have brought forward, appear, to me, to be sufficient to refute the doctrine of Haller, and of Dr. Wilson Philip, and others of that school.

It must, however, be admitted, that Dr. Young's theory is too mechanical, and that he loses sight of the

influence of vitality in arteries, to which many phenomena must be referred. Hence, a more enlarged view of the circulating forces becomes necessary.

The affections of the arteries, and more especially of the capillary arteries, from moral causes, as well as from narcotic and sedative agents largely applied to the brain and nervous system, and also the action of some stimulants, are inexplicable according to a merely mechanical theory of the circulation. But these affections do not support the opinion of an independent muscular action; nor of alternate systole and diastole of arteries, in favour of which hypothesis, Dr. W. Philip has laid a series of experiments before the Royal Society, which are repeated in his Experimental Inquiry, and also in his introduction to his treatise on Symptomatic Fever, (fourth edition,) and in his essay on the Circulation, published in the Transactions of the Medico-Chirugical Society, vol. xii. part 2; some of which he calls *experimenta crucis* in favour of his opinions. We shall soon see how they are to be explained in conformity with the facts already adduced, and with the forcible reasoning of Dr. Young.

I have acknowledged that the latter carries his mechanical principles too far. In his opinion, the vitality of the arteries has no other influence than to increase their elasticity. But if we admit, as we must reasonably do, that organic structure depends on the agency of the living principle or force, it follows, that arteries must be subservient to its laws and influence. We ought not, however, to forget that a dead artery, until it begins to suffer decomposition, is nearly as elastic, if not equally so, as a living one; and that owing to its mechanical elasticity alone, it can be made to pulsate when injected by repeated jerks of any fluid.

Every phenomenon of the circulation of the blood in its usual or normal course, which depends on arterial

pressure, may, indeed, be explained by the elasticity of the arteries. But, although for the wisest ends, these vessels are very insensible to nervous excitement and impressions, and, consequently, are not exposed, like some other organs, to be agitated and disturbed by the passions, yet, we have abundant proof that, like most of the living structures of our complicated frame, they may, under certain circumstances, be influenced by affections of the brain. In no other way can we account for either the turgescence or collapse of portions of the blood-vessels from mental causes, or from certain stimulants and sedatives, the first action of which is on the stomach.

It is necessary to keep these facts in mind, in order to understand the action of the causes of fevers and some of their phenomena, as well as those of other diseases in which there is great disturbance in the circulation; and yet it is to a very limited number indeed of diseased appearances, that this principle refers.

Professor Schwann, whose name must be familiar to every well-informed physiologist, made an experiment by which he hoped to prove a vital contractility in arteries; and it is quoted in that classical work, the Elements of Physiology, by Dr. J. Müller, as a proof of the proposition. This supposed vital action consists in a slow contraction of the arteries on the application of cold, and on their regaining their former diameter when the cause ceases to act. After having spread out the mesentery of a living toad under a good microscope, Schwann allowed some drops of very cold water to fall on it, and observed that the minute capillary arteries began immediately to contract themselves, until they were reduced to one-third of their usual diameter. Some time after, they began to be dilated, and ultimately regained their original dimen-

sions. This was considered as the specific effect of *vital* elasticity. But all vital movements are depressed by cold; whereas the contraction of the capillaries in this experiment seems to show an increase of action, irreconcilable with the action of cold on all other vital movements. To explain the fact, we must recollect that iced or very cold water, would necessarily occasion the blood to be condensed, on the principle on which cold acts on all fluids; and we know that in proportion as the blood is diminished in volume, by any cause whatever, the capillaries contract their diameters, owing to their perfect elasticity. Therefore, in this case, they would necessarily accommodate themselves to their contents; and in proportion as the blood regained its original temperature and volume, in the same proportion and ratio the capillaries would become again distended. It is on this principle that cold acts on the capillaries of the skin, stomach, and all other living parts; not by stimulating them to contraction, nor by spasm, but merely because the vessels accommodate themselves to the diminished volume of the blood, no matter from what cause this diminution originates, whether it be from cold, or from hæmorrhage, or from weakened action of the heart, as in nausea or fainting, or from inanition. The paleness and shrinking of the skin and of the whole surface and volume of the body, in such cases, are physical, not vital effects.

I deem it of such importance to practical pathology, especially as regards both the doctrine and treatment of fever and inflammation, to point out the fallacy of Haller's and Dr. W. Philip's doctrine of the circulation, that I cannot avoid adding another well-known fact to those already adduced, which seems of itself to be sufficient to destroy the notion of muscular action in the arteries; I allude to their ossification. In the first volume of the Medical Communications, Mr. Watson

gives us an account of the dissection of the body of a very gouty gentlemen, a Mr. Middleton, in whom the aorta, from the diaphragm to the iliac arteries, *was wholly ossified.* The arteries of the brain, as well as of the extremities, are often attacked by this disease in old and gouty subjects, and yet they live several years notwithstanding. Whoever is desirous of consulting many parallel facts and observations, will find them in the *Opera Minora* of Haller, t. i. p. 230. From this it appears that the circulation of the blood may be sustained for years by the impetus of the heart alone, or, at all events, with very little assistance from the elastic or contractile force of the arteries. Many other facts, derived from comparative anatomy, show that the chief propelling force is in the heart alone; for in a number of fishes the aorta is firmly attached to, and runs through the arch of the spinous processes of the back bone, and in the sturgeon the chief artery is a cartilaginous canal.

But I come now to the experiments of Dr. W. Philip, and which were executed by himself and several eminent medical gentlemen: some of these he considers as perfectly conclusive confirmations of his hypothesis.

In his paper on the Circulation, in the twelfth volume, p. 2, of the Medico-Chirurgical Transactions, after some severe animadversions on the intellectual faculties of those who think that the circulation of the blood is supported by the muscular power of the heart and elasticity of the arteries, he asks, "Have those who maintain this position made even the *rudest calculation* of the degree of resistance to be overcome in driving the blood through two capillary systems at such a rate, that, in any given time, the same quantity shall be delivered by the veins, which is thrown into the arteries?" (p. 401.) And this question, Dr. Philip asks in the year 1823, that is, fourteen years after the publication of Dr. Young's

Croonian Lecture, in which there is the most beautiful display of deep and convincing mathematical calculation on these very subjects. There is an experiment which is easily made, and which, when executed on a healthy subject, gives a good idea of the impetus which blood receives from the heart, alone, at each contraction of its left ventricle: on crossing one leg over the other, and loading the foot of the upper leg with weights, it will be found, says Burdach, that the foot loaded with fifty pounds avoidupois, will be raised by each pulsation of the popliteal artery. The impetus, therefore, which the blood receives at that part of the arterial system, surpasses fifty pounds weight; but, as the popliteal artery cannot receive more than the fifth part of the general impelling force of the heart, the whole force may be calculated as being more than equal to two hundred and fifty pounds. As to the various resistances to be overcome by such a pressure, recourse must be had to the calculations of Dr. Young. It is not perhaps out of place to remark, that the above-mentioned experiment affords a good practical way of ascertaining the comparative force of the heart's action in different individuals and in the same individual in different states of health, provided he be not too fat for such a trial.

Dr. W. Philip, after expressing his wonder that experiments had not been made to ascertain the amount of resistance to the impelling power of the heart, introduces his chief experiment, with the following syllogism: "If the motion of the blood be maintained by the power of the heart alone, it will, of course, cease when this power is destroyed." And he then describes many experiments, which prove that the blood in the capillary arteries, when retained in them by ligature on the aorta, and after the extirpation of the heart, is moved forward with as much celerity as when they receive blood without interruption from the heart.

Now, in answer to the question arising out of the syllogism, it may at once be answered with the affirmative word—*certainly*. The premises and consequence must be admitted. But it may be asked, in return, to what does the question apply? Surely not to the conclusions of Dr. Young. In no part of the Croonian Lecture does he attempt to prove that the circulation of the blood depends solely on the heart. In this respect he argues much more correctly than Dr. J. Müller and many other excellent physiologists of a recent date.

What Dr. Young proved, in 1809, was, that the circulation of the blood depends nearly as much on the constant pressure of the arteries, as on the *vis a tergo* given to the blood by the action of the heart. The experiments of Dr. Wilson Philip, just alluded to, tend to prove this fact and nothing else. But he gives a very different explanation of them. 'A ligature,' he says, 'was thrown 'round the vessels attached to the heart of a frog, and 'the heart was then cut out. On bringing the web 'of one of the hind legs before the microscope, I found 'the circulation in it vigorous; I could not distinguish it 'from that of a healthy frog. It continued in this state '*for many minutes, and at length gradually became more* '*languid*. A rabbit, about two months old, was killed by 'a blow on the occiput; the chest was then laid open, and 'a ligature thrown round the aorta; part of the mesentery 'was now brought before the microscope, and both of the 'gentlemen who assisted me, Mr. Shepherd, and myself, 'saw the blood moving in it with great velocity. I had 'many times, with the assistance of the microscope, seen 'the circulation in the healthy rabbit, and could not per-'ceive that the loss of the power of the heart at all affected 'it, *for the space of many minutes*.'

Without pretending to express doubt as to the accuracy of Dr. Wilson Philip's experiments, it may be remarked,

that similar experiments were made by Dr. J. Müller, who states that after the artery was tied, the motion of the blood in the capillaries *ceased* almost instantaneously. Supposing the correctness and fidelity of the experimenters to have been equal, the difference of the results may be accounted for, by a difference in the vitality of the animals selected for the experiments. The experiments of Müller are narrated in the Isis for 1824, which I do not possess; but they are referred to by Burdach in the sixth volume of his Traité de Physiologie, p. 309; he says, "Le courant (du sang) s'àrrête presque instantanément dans les capillaires, aprés la ligature de l'artére, et l'on n'observe plus alors qu'une oscillation lente, *due à la pression des parois* qui cesse elle même bientot."

Explanation of the Experiments just cited.

First, let us call to the recollection of the reader an experiment invented and executed by the late justly-celebrated John Hunter, and which is quoted by his distinguished pupil Abernethy, in his Physiological Lectures. The object is to prove the great contractile power of arteries, which arises from their vitality and organic structure. Mr. Hunter allowed a horse to bleed to death, and on examining its body afterwards he found that the aorta had contracted itself to no less than one-tenth of its diameter; and that the arteries which were of the diameter of the radial artery in man, had expelled every particle of blood. During this state they never were relaxed like muscles, nor like the ventricles and auricles of the heart after their contractions. Hence, it follows, that if the aorta be tied, and the heart is either cut out or allowed to remain, the elasticity or contractile power of the arteries will be sufficient to propel the

blood forwards through the capillaries into the veins, the elasticity or contractile tonicity of these vessels being sufficient for this purpose. But the duration and appearances of this progressive motion of the blood in the capillaries will be different, according to one or other of the three following conditions. If all the great blood-vessels of the heart are tied with a ligature, the contractile power of the arteries will only be exerted for a limited time, and in a limited degree; for the accumulation of blood in the veins, from which it cannot escape, will at last produce such a resistance as can no longer be overcome by the contractility of the capillaries; if the aorta alone is tied, and the heart cut out, and, consequently, the veins cut through, the capillaries will be seen propelling the blood, until the last particles are discharged; if the heart of the frog be cut out, and neither the arteries nor the veins be tied, we shall see the blood in the capillaries move both backwards and forwards after the loss of much blood, because there is no longer any resistance to its motion either way.

The experiments by which Dr. W. Philip has endeavoured to prove the dependence of arterial action on nervous energy, have been repeated, multiplied, and varied by other eminent physiologists, but not with the same results. On this subject read Dr. J. Müller's Elements of Physiology, translated by Dr. Baly, vol. i. p. 232, where he expresses himself thus:

'The numerous experiments of Dr. Wilson Philip 'also fail *completely* to establish the influence of the nerves 'on the circulation in the capillaries. The retardation or 'cessation of the circulation in the capillaries which he 'observed when opium or infusion of tobacco were applied 'to the brain and spinal cord, or when these parts were 'suddenly destroyed, was dependent on the effect which 'was produced simultaneously on the heart. Koch (see

'Mickel's Archive, 1827, p. 443) made an ingenious and 'simple experiment, with a view to determine the same 'point. He amputated the leg of a small frog, and found 'that the motion of the blood in the capillaries of the 'web of the foot continued for three minutes only. He 'then, in another frog, divided all the parts of the thigh 'but the ischiatic nerve, by which he left the limb 'attached to the body; and he now perceived motion in 'the capillaries for the space of a quarter of an hour. I 'have repeated this experiment, but not with the same 'result. The motion of the blood in the capillaries con-'tinued about ten minutes, when the limb was com-'pletely separated from the body in strong frogs; and there 'was no difference in respect to time, when it was left 'attached by the ischiatic nerve. In this experiment an 'error may be induced, by the frog retaining the power of 'producing voluntary contractions of the muscles of the 'limb as long as the ischiatic nerve maintains its con-'nexion with the nervous centres: after each contraction 'of the limb, a slight motion in the capillaries is perceived, 'but the cause of this is evidently mechanical.'

Dr. Müller sums up the evidence of numerous experiments on this subject, in the following words.

'From the facts which we have detailed, it appears 'most probable that the nerves do not really assist in 'carrying on the circulation in the capillaries; athough it 'is certain that nervous influence is the principal cause of 'the accumulation of the blood in the capillaries of cer-'tain parts, during the state of vital turgescence. The 'observation, that in a frog much exhausted, the impulses 'communicated to the blood by the feeble contractions of 'the heart are perceptible in the capillaries, proves that 'no other force than that of the heart is required to sup-'port the circulation.'

It is a sound rule in philosophy, which is applicable to

physiology as well as to every branch of natural knowledge, not to invent or assign more causes for the explanation of an event or phenomenon, than what are sufficient to account for its appearance. If the mechanical impulse communicated to the blood by the systole of the ventricles of the heart, and the pressure exercised on that fluid by the elasticity of the arteries, be quite sufficient to account for the general phenomena of the circulation, it is not reasonable to seek for imaginary causes, however plausible they may be, or however difficult it is to reconcile some anomalous appearances with the general doctrine. But there is a vast difference between the doctrine of the circulation as founded on a correct calculation of forces, (which, on that account, I shall, in the following Commentaries, call the dynamic doctrine,) and the admission, that cerebral force, or nervous energy as it is commonly called, may, and in reality does, often affect the whole, as well as part of the vascular system.

For the wisest of purposes, the blood-vessels are scantily supplied with nervous filaments; anatomists have not yet detected them. The reason seems to be, that the regular current of the blood in the arteries and veins is not to be exposed to hourly, or even accidental agitation from moral causes; and yet the reddening of the face and neck from shame, or from sudden joy, and now and then from surprise; the sudden paleness of the skin, and sense of coldness, from fear and terror; the turgescence of certain organs from sexual desire; have been justly considered, I think, as proofs of nervous or cerebral influence on portions of the vascular system; for these phenomena being local, cannot in reason be ascribed to the contractions of the heart. I do not mean to deny that the sense of shame which produces blushing, however slight it may be, does not affect both the rhythm and the force of

the heart's action. The phenomenon is of so fleeting a character, and the sympathy with the agitation and feelings of the timid person so great, as to prevent all examination of the pulse at such a moment. But I suspect it would be found to be altered from its natural state. Admitting this power, it leaves the local congestion in the face and neck unexplained and inexplicable, except on the principle of nervous influence. Nor can it be doubted, I think, that the blood-vessels, like all organic structures, are endowed with their due proportion of the vital principle; and that, consequently, their normal functions may be altered by the stimulant and sedative agents which affect living organs, both when directly applied to them, or indirectly through the brain and nervous centres. But all such affections are reducible to two in number, namely, the increase or the diminution of the natural functions of the vessels; and as this consists solely in their elasticity, it is this function which is either increased or weakened by the force or diminution of cerebral influence. It is not a new action, like muscular contraction and dilatation, nor a spasmodic constriction, which is generated; the structure of arteries does not admit of such effects as these.

As to local congestions of the capillaries, arteries, and minute veins, and their numerous causes, as well as their *modus operandi*, the subject is one of great extent and importance in practical pathology. It embraces the doctrine of inflammation in general, and the distinction between that affection and congestion; and it also leads to an inquiry of great interest, namely, the theory of all irritants, mechanical, chemical, and mental. For, although it is well known that these agents have few qualities in common to all of them, nevertheless, they produce similar effects on vascular parts of the living body, and, consequently, it is to be suspected that they act on one general principle.

Our doctrines of inflammation, congestion, and fever, cannot be improved until the operation of such causes has been minutely and successfully examined.

I cannot conclude this commentary without offering an apology to those physiologists of the present day, who, like Burduch, J. Müller, Tiedemann, Alison, and Mayo, have either totally abandoned Haller's notion of a *vis insita* or muscular irritability in arteries, as quite untenable, or who, at least, question its validity, but still remain in doubt. To them the discussion may appear to be superfluous; but it cannot, surely, be unnecessary to endeavour to confirm an important truth, so long as it is doubted or opposed by men of celebrity and talent. Some of the late volumes of the Philosophical Transactions show with how much pertinacity Haller's notions are still maintained. Bostock also, in his Physiology, which was published many years after Young's Croonean Lecture, prefers the fanciful speculations of Dr. Cullen, concerning the action of the capillaries, to the more correct and convincing views and arguments of Young. But even those whose opinions coincide with Young's, seem to me to have arrived at their conclusions, not in consequence of his powerful reasoning, for his name is not mentioned by one of them as the true founder of the doctrine, but from the slow and gradual accumulation of facts derived from microscopic anatomy, and from the chemical analysis of the muscular fibre, when compared with that of the middle tunic of arteries.

APPENDIX

TO THE

PRECEDING COMMENTARY.

ON EXERCISE AND INACTION.

At page twenty-seven I promised to add a few remarks on exercise to those which are to be found at that place; for, although the subject is a hackneyed one, still it appeared to me that there was room for a few observations, not only on the general theory of its action, but also on some errors which are daily committed in using it. I am quite aware that it is in vain to offer advice on this subject to nearly three-fourths of mankind; not from the suspicion that they are unwilling to receive it, but, merely, because they are not masters of their time and actions. I address myself, therefore, to those who can command both, and who, if they will not try to live as long as possible, must be accused of unbelief or obstinacy.

The first effects of bodily exercise being the mechanical compression of nearly one half of the capillary arteries and small blood-vessels of our frame, and the consequent forcible propulsion of a larger quantity of blood to the heart than usual; the subsequent changes which take

place in the circulation and its functions must be traced to that cause. These effects are more numerous and complicated than is commonly conceived, except by medical men who have attentively considered the subject.

Regular exercise, when moderately and gradually increased, may strengthen the structure of the heart, on the same principle that any muscle of the human body is strengthened by frequent and moderate use, that is, more blood than usual being made to circulate through its substance, the process of nutrition is increased, and the fibres of the heart necessarily augment both in size and number. But in order that such a process be successful, the laws of excitement must be carefully observed.

The natural stimulus to each of the muscular cavities of the heart is about one ounce and a half of healthy blood. The normal functions of the heart require that this stimulus should be applied to the internal surface of each cavity, about seventy-six times per minute on an average for adults. A less quantity, or a less frequent repetition of the stimulant, produces *direct* asthenia of the heart, and of every organ of the body, as far as their health depends on nourishment. This is exemplified in various diseases, as in chlorosis, dropsy, profuse hæmorrhage, and in all cases of anæmia. A larger quantity than usual, or a more frequent application of the stimulant, independently of the danger arising from congestion, exhausts the vitality or *vis insita* of the heart by overstimulation: producing numberless affections of *indirect* asthenia. It is for this reason that violent exercise produces many obstinate chronic diseases, and even death itself, without any previous derangement of structure.

Instances of the first kind of such bad consequences are unfortunately not infrequent among young people who have not as yet attained their full developement and strength. With such individuals, a long and fatiguing

walk, or dancing too much, produce indirect asthenia of the heart; some of the complicated effects of which are often misunderstood and attributed to inflammation of the spinal cord. Instances of the second or fatal kind, are met with among soldiers (generally young recruits) in long and forced marches, and among seamen during protracted storms; the *vis insita* of the heart being irrecoverably exhausted.

Again, inasmuch as all secretions and excretions, the function of respiration, and, finally, the organic, as well as the chemical composition of the blood, depend much on the normal state of the circulation, it follows, that they may be all affected for good or for evil by exercise according to its violence and duration.

A common error is frequently committed from ignorance, or from false notions, by those who have a full command of their time and pursuits. Many unemployed men of wealth imagine that if they dedicate several hours, consecutively, in the early part of the day, to a fox-chase, to a stag hunt, to a game of cricket of many hours' duration, or at tennis, or in fencing, or any other gymnastic exercise, they have done that, which contributes much to health, at least for one day; judging perhaps by the keen appetite, which so great a waste of solids and fluids occasions. The consequence is, that the remainder of the twenty-four hours is apt to be spent in nearly complete inaction and hurtful indulgences. Many strong and healthy young people may unquestionably derive benefit from such exercises in the open air, but that many others are injured by it is certain, for the reasons assigned, as well as for others I am about to mention.

In the preceding commentary I have shown how the heart and larger arteries may be suddenly disorganized by over-distension, and in the present appendix I have

endeavoured to point out the consequences of over-stimulation.

Let us now turn our attention to another consequence of violent exercise. One of the many important functions with which we are mercifully provided for the maintenance of health, is insensible perspiration, by which about eighty ounces of useless chemical irritants are daily eliminated from the blood; but it is an error to suppose that sweat, which is the natural consequence of violent exercise, is equally beneficial, or that it acts on the same principle. I am aware that Dr. J. Müller and other eminent physiologists consider perspiration and sweat to be functions of the same kind. It is, therefore, with diffidence that I offer some arguments in proof of a contrary opinion. I do not deny that both insensible perspiration and sweat issue out of the same canals and sudatory organs, and yet the causes of their appearance, and the matters eliminated by them, are different. When sweat is excited, no matter from what cause, that is, whether from violent exercise or from the action of diaphoretics, or from atony, the vessels are preternaturally dilated. When it arises from exercise or stimulating diaphoretics, the blood being propelled with more than usual violence to the surface, the vessels are forcibly dilated. But in the case of colliquative sweat, or when debilitating diaphoretics, such as the tartrate of antimony, have been too long employed, the tonicity of the arteries is weakened, and the sudatory canals become widened from defective elasticity. Insensible perspiration carries out of the system a volatilized fluid, together with a very large quantity of carbonic acid and ammoniacal salts; but sweat, in addition to these, eliminates a part of the serum, some albumen, which is dissolved in it, and osmazome, from which it is natural to infer, that the blood is deprived of nutritive

particles ; and this explains the chief, though not the only cause, of the emaciation of the body, under the operation of protracted diaphoresis.

But, it will probably be said in favour of severe exercise, that physicians frequently order it with success, as a means of cure in many obstinate complaints. No one is more ready to consent to this proposition than I am. I have seen some diseases of the skin, of the most opposite character, which had resisted the influence of medicaments, disappear entirely under severe exercise, when combined with restricted diet : and I acknowledge that much benefit is often to be obtained from it in obstinate chronic rheumatism, and in bilious and gouty affections. But these facts and arguments cannot be adduced in favour of severe exercise, as a general means of preserving health, any more than it would be to say that cinchona ought to be an article of daily food because it cures many diseases.

The cutaneous and rheumatic affections which yield to severe exercise, are in general more easily cured by baths of hot air impregnated with sulphuretted hydrogen. The fact is, that it is not the stimulus of exercise which does good in these cases, but the elimination of chemical irritants along with the sweat. When, therefore, a bath of sulphuretted hydrogen is to be employed for the relief of such disorders, its temperature ought to be raised to about 150° Fahr. ; a temperature which is easily borne when the body is only surrounded with air.

It can surely be easily understood why severe muscular exertion is dangerous to all persons whose structure has been imperfectly developed, as, for instance, with those in whom the lungs and thoracic cavity do not bear a just proportion to the aortic system, and to those also who have an apoplectic structure. The probable consequences must, I think, present themselves to the mind

of every medical man, and need not, therefore, be dwelt on. But I think it necessary to make some reflections on that disease which suddenly attacks young people of a weak frame in consequence of imprudent exercise.

Its most characteristic symptoms are pains in some part of the spinal column, a sense of great languor and fatigue, and more or less muscular weakness in the lower extremities, the pulse being generally quicker and smaller than usual, together with some disturbance in the digestive organs.

The weakness and quickness of the pulse, which follow asthenia of the heart, whether produced directly by debilitating agents, moral or physical, have been sufficiently explained in the preceding commentary. As to the pain in the spine and in the lumbar and dorsal nerves, and the atonic state of the muscles of the lower extremities, it is well known, that they are generally ascribed to local inflammation of the medulla spinalis, or of the cartilages, ligaments, and bones of the vertebræ. Now, I do not deny that such lesions may arise in the course of the disease; but my experience of it justifies me in saying, that in four-fifths of the cases, there is no local injury whatever at the commencement of the disorder. I consider the neuralgia, which is always of a dull but teazing kind, to arise from a defective nutrition of the medullary substance, an event which naturally springs from the exhausted energy of the heart and derangement of the chylopoetic viscera.

The recovery of a patient in this disorder cannot be otherwise than slow. The heart will not bear to be stimulated, and it must not be debilitated; and, from the nature of its organization and the ultimate object of its function, it never ceases to be in action, and consequently, it cannot have that repose which we know to be necessary for the recovery of the exhausted *vis insita* of a muscle.

Corporeal and mental rest are clearly indicated, and so are strengthening remedies, but the latter must be of such a kind as not to stimulate the heart to frequent contractions. A nutritious diet, which is of easy digestion, and which is given in as large quantity, and as often, as the stomach of the individual can bear it; the employment of very minute doses of quinine, sulphuric acid, or the vinum ferri, during the day, and a small opiate at bed-time, to procure sleep when that is wanting; are the chief remedies which assist the efforts of nature. If palpitation of the heart occurs frequently, and, indeed, when its action is too frequent, the application of the extract of belladonna as a plaster to the chest is useful. If the patient's bowels are relieved once in thirty hours it is sufficient. Purging is hurtful.

A good rule for the employment of exercise, with the view of prolonging life, consists in taking it, not once, nor even twice, but several times a day; for by so doing, the force of the circulation is sustained, and organic structure safely increased, on the principle on which all parts are nourished.

Bodily inaction ought not to be persisted in for upwards of four hours consecutively, if perfect health is the object, except during sleep. And for people in health this tempting state of repose ought not in general to be prolonged above seven hours. For the last forty years of my life, I have made it a rule not to remain longer quiescent, during the day, than three hours at a time; and I attribute the good health I enjoy at my advanced age, chiefly to this cause; for I must confess that I scarcely owe anything to dietetic rules, having never been a rigid observer of them. For people who are in good health, there is only one dietetic rule which is truly useful; namely, not to eat too much, nor too great a variety of food at any meal. As to diseases and

individual constitutions there are no rules of diet which are universally applicable.

If it be argued that the climate of Great Britain during more than half of the year does not permit people of delicate health to take that quantity of exercise in the open air which would be useful to them, I answer, that a small apartment is not an obstacle to the taking due exercise.

If it be said that the rules and practices of exercise which I recommend, are so many acts of intolerable penance, that few people will submit to them; all I can answer, is, that assuredly they are not agreeable pastimes; but he who will not believe that the maintenance of health demands that a due balance be daily observed between the quantity of *ingesta* and *egesta*, between the strength of the heart and vascular system, on the one hand; and the quantity of fluids they are obliged to circulate, on the other; and between the unceasing formation of numerous chemical irritants, that are inevitably produced and absorbed during the digestion of food, and other natural processes; and the elimination of the same; (all of which balances are promoted by exercise,) must not complain either of bad health or a short life.

COMMENTARY SECOND.

ON THE DOCTRINES AND TREATMENT OF CONTINUED FEVERS OF A TYPHOID CHARACTER.

An account of typhous fever and its treatment in London about the year 1790—Under what circumstances it became contagious—Details of practice—Account of the first great innovation in the treatment of typhous fever by Dr. Millar of London, and Dr. J. Clarke of Newcastle—Detailed description of Dr. J. Clarke's practice—Its temporary reputation and adoption by some eminent London physicians—Critical remarks on it—Its gradual loss of reputation—Brownianism in general—German modification of it—Some of its excellencies and defects—The school of the *philosophizing physicians of nature*, Schelling, Killian, and Oken—Instances of their discoveries in transcendental philosophy—Anecdote explaining the reasons why the author, in the midst of a practical life, was obliged to recommence his studies—A general rule for testing doctrines of disease—Of Rasori's Brownonian practice in 1799, and of the contagious typhus of that period in Genoa—Circumstances which caused Rasori to abandon both the doctrines and practice of Brown—His invention of the hypothesis of counter-irritation, with an explanation of it — Diathesis di stimulo—Diathesis di contrastimulo—Instances of practice agreeably to the doctrine—Rise and progress of the last great school of innovators in the doctrine and treatment of typhoid fevers, namely, that all such diseases are symptomatic of visceral inflammation, and ought to be treated accordingly—The false reasoning on which the hypothesis is founded—The true nature of the inflammatory appearances explained—The diastrous consequences of its application to practice—Cases in illustration—The author's doctrine of typhoid fevers, and its application to practice, with various details.

BEFORE entering on the history and examination of the extraordinary changes of opinion and innovations in

practice, which have succeeded each other so rapidly within the last fifty years, in regard to the doctrine and cure of continued fevers of an adynamic character, I trust that it will not be deemed irrelevant to the subject, if I commence with a short account of the general treatment of the same diseases, as it was practised in London when I first entered on a public and practical life, that is, about the year 1790.

In the commencement of that career I had the good fortune to be elected one of the physicians to a public dispensary in Featherstone Buildings, Holborn. By its laws and regulations, such patients as were unable to come to it for advice and medicines, were to be visited by the physicians and surgeons of the institution at their own houses.

The circle of our visits comprehended the greater part of St. Giles, and all the narrow, confined, humid, and dirty courts, which abound on both sides of Holborn, together with the purlieus of Liquorpond Street, &c.

I do not recollect one year to have passed over, while I was attached to that useful charity, in which typhous fever did not originate almost every spring and autumn; and, also frequently during winter, at one and the same time in several distant spots; but always in dirty and narrow lanes, and in damp courts, in which the wretchedly poor inhabitants were crowded together in low confined apartments. This was more particularly the case in some of the narrow lanes and courts of St. Giles, which were chiefly inhabited by the lowest classes of Irish labourers, and by the disorderly of both sexes, who were reduced to the last stages of vice and destitution.

Some years afterwards, on being chosen physician to the Westminster Hospital, I had frequent opportunities of observing similar facts among the poorer classes in

Pimlico and its neighbourhood; for, at the period I allude to, that parish bore a very different appearance from what it exhibits at present. It had not then been well-drained; and as many parts of it were lower than the river at high water, it partook at times of the unhealthy malaria of marshy land; so that typhus and bad intermittents frequently prevailed among the poor.

The recollection of these facts often brings to my memory many thoughts which presented themselves to my mind in those days, relative to the necessity of a well-regulated and duly-authorised medical police, or board of health, not only for London, but for all our great cities, and more especially for some of the densely-peopled manufacturing ones, in which there was a disgraceful neglect of the health of the inhabitants, as far as the free ventilation of air and removal of filth, and other local causes of malaria are concerned. But it is foreign to my present purpose to dwell on this subject.

The cases of typhus which were met with every spring and autumn in the localities just described, did not appear to be contagious on their first appearance. But when several members of the same family laboured under it, and shut themselves up in close apartments to keep out the cold, as they said, a poison was generated by the confined exhalations of their bodies, which soon affected many of the neighbouring families; but still the fever seldom spread far. It commonly assumed the character of typhus mitior, or the mildest form of nervous fever, which has been so well described by Gilchrist and many others. When a very cold and damp autumn succeeded a very hot summer, the fever had a severer character, and was frequently seen with every symptom of typhus gravior; being then complicated with bad bilious symptoms, also with petechiæ and vibices. But I ought to remark that I do not recollect to have

seen the truly bilious synochus, such as Sydenham describes, to have prevailed as a general epidemic, although a few solitary cases occurred now and then. In general, the disease was easily cured, when the brain was not too much affected, in the beginning, with profound stupor and coma. Violent delirium in the first stage, though an untoward symptom, did not render the disease incurable. Leeches to the neck, and blisters on the inside of the thighs, and between the shoulders, with a free admission of cool, and even cold air, and the frequently washing the head, face, and neck, with cold vinegar and water, together with the internal administration of nitrate of potash, and the liquor ammoniæ acetatus, to which were added the tartrate of antimony and calomel in very small doses, frequently repeated, were the most effectual remedies for diminishing the cerebral affection. Delirium in a moderate degree, was never considered as an unfavourable symptom. In no instance did I ever employ the lancet for its removal; I never deemed it necessary. Violent headache and moderate delirium, which happen in the very commencement of typhus, generally yielded to a large blister between the shoulders, and to the other remedies just enumerated. But when the disease began with profound coma, its termination was almost always fatal.

The treatment of typhous fever by almost all the practical physicians of London, at the period I am alluding to, was nearly uniform. It was the same which had been handed down from the time of Sydenham, the founder and great ornament of British medical practice. The physicians to whom I was in the habit of applying for consultation and admonition in private practice, were, the father of the late Dr. Warren, Dr. David Pitcairn, and Dr. Reynolds, all of them eminent as safe and successful practitioners.

With one accord, they all abstained from blood-letting at every period of the disease, except under very particular circumstances. Emetics and purgative remedies were sparingly employed; the practice agreeing in this respect with the advice of Dr. Cullen. The bowels were relieved when necessary by enemata, rather than by the uncertain, and frequently injurious effects of any common purgative medicament. In the first stage of the disorder, the plan of treatment was, so far, strictly antiphlogistic, that all tonics and stimulants were avoided, while refrigerants, and the saline diaphoretics, with small doses of antimonial wine, or of tartar emetic, or of ipecacuanha, were freely and frequently given. Some, who had great faith in the compound powder of contrayerva as a mild diaphoretic, gave that also in the first stage. Blisters were applied to relieve headache and delirium; and cool diluting drinks and a free admission of air were deemed indispensable. But in the second and subsequent stages, the rapid exhaustion of the strength of the patient, was the chief object of anxiety; and it was counteracted, as much as possible, by mild nutritive drinks, and by wine given in suitable doses, and also by a decoction of the Virginian snake root, and in many cases by an infusion or decoction of cinchona, to which the saturated lemon juice, or Riverius's draught, was almost always added when the skin was dry.

To allay subsultus tendinum, tremor, irritation, and nervous languor, recourse was had, with variations, to valerian, camphor, musk, Hoffman's ether, (an excellent remedy,) to laudanum, and very often to the mithridate or theriaca, or, in lieu of it, to the old confectio cardiaca, with or without opium, according to circumstances.

The varieties of the fever, which arose from seasons, vicissitudes of temperature, and individual idiosyncracies,

necessarily produced a variety in this practice. The bilious form of typhus demanded the free use of emetics, and afterwards of calomel and purges; while the petechial typhus called for a liberal use of the mineral acids, and for larger doses of wine, which was found to be the best of all febrifuges.

It has been already mentioned that blood-letting was seldom employed by the London physicians and practitioners at the period I am alluding to, that is, from 1792 to 1804. On the contrary, it was generally condemned as a dangerous remedy. Even in the cases in which typhus now and then commenced with catarrhal symptoms and pleuritic pain, the lancet was sparingly used; ten ounces of blood being the largest quantity which was abstracted at once. It was seldom repeated a second time: and yet I have not seen typhus fever in all its forms more successfully treated than it was at that time. I have lived long enough to have witnessed many strange innovations in the mode of treatment, and I am sorry to add, that, judging by the results, they cannot be counted as improvements.

The first great innovation in the practice recommended by Sydenham, and by the scholars of Boerhaave, Van Swieten, Hoffman, Cullen, and a whole host of excellent authorities, originated with two British physicians of eminence, Dr. Millar of London, (see his *Observations on the Prevailing Diseases of Great Britain*, 1770,) and by Dr. John Clarke of Newcastle, (see his *Observations on Fever*, 1790; *and also his Observations on the Diseases which prevail in Long Voyages, &c.*, 1792; *second edition.*)

The innovation consisted in giving from one to two ounces of cinchona in substance, together with a strong decoction of the same in cinnamon water, in the very first stage of continued fevers, even if the pulse was hard, as well as quick, the heat great, the skin and

tongue dry, and the urine scanty. Violent headache did not prevent the practice.

On Dr. Millar's testimony I do not rely much. He never was in extensive practice; and he was better known among his contemporaries for the oddity, than for the soundness, of his opinions. But in Dr. J. Clarke of Newcastle, we have to deal with a very different personage. He had been regularly educated to medicine and surgery in the University of Edinburgh. During many years he had served in the navy, and had often seen the typhus gravior (the hospital and camp fever) in a variety of climates. After retiring from the service, he established himself in Newcastle, and was appointed physician to the infirmary and dispensary of that city, in which the typhus often raged afterwards as a contagious epidemic; consequently, his opportunities of observing that disease were great. His history of symptoms is correct and good; and his precepts for the health of seamen, and the prevention of contagion in the navy and besieged cities, are judicious and useful. But how to reconcile the results of his practice in fever, either with that of preceding experience, or of the totally opposite method of treating the same kind of fever which followed soon after his death, is scarcely possible. The description of his treatment shall be given in his own words, that I may not be accused of misrepresenting it.

'I have shown' (says Dr. Clarke) 'the safety and 'advantage of exhibiting the bark early in continued 'fevers, which occurred in my practice in this kingdom. 'From 1770 to 1791, I have attended about thirteen 'hundred patients in all the varieties of continued fever, 'and I do not remember that above four cases have come 'under my care, where the medicine failed.' It may here be remarked, that this is nearly the same kind of confident language, which Rasori, Dr. Armstrong, and

others employed, when they recommend forty ounces of blood to be taken away at once, and repeated in the first stage of the very same kind of fever! But I am anticipating a little. In another part of his Observations he adds, 'I can affirm that within these few years my own life 'has been twice preserved by taking it (cinchona) 'largely and liberally on the first day of confinement, in 'fever attended with such symptoms as would have de- 'terred many physcians from prescribing it. And, did 'I think it necessary, I could adduce testimony of several 'medical gentlemen in this place, (Newcastle,) who 'have found equal advantage from it, when exhibited in 'the same manner, either in their own cases or in that 'of their relations or patients.' I ought to have observed that the preceding statement of Dr. Clarke relates particularly to the treatment of contagious typhus or hospital fever, in which disease it has been proved incontestibly of late, that local inflammations are generally detected after death. Nevertheless, in all the stages of this fever, Dr. Clarke gave two ounces of a strong infusion of cinchona in boiling cinnamon water every half hour; or an infusion of the same strength, i. e. an ounce of the cortex to ten or twelve ounces of fluid; to which he added occasionally conserve of roses, one ounce; diluted sulphuric acid, one drachm; and one ounce of French brandy; and to such mixtures he also added the powder of Bark, as soon as he conjectured the patient's stomach could bear it. In the advanced stages, he gave, in addition, the Virginian snake root and ammonia.

I well remember, that the publication of Dr. Clarke's work produced a great sensation among the London practitioners of medicine. Most of us had been pupils of Cullen, and had imbibed his doctrines, and imitated his practice. Few could be induced to try it at first. The mild diaphoretic and antiphlogistic plan was

undeviatingly pursued by the greater number, for the best of all reasons, because it was found, generally, to be safe as well as successful. Nevertheless, some physicians of reputation and experience began to make cautious trials with the tonic plan, and their reports were undoubtedly in favour of it. Dr. John Hunter, Dr. Lettsome, Drs. Huck and Sims, and some others, assure us that they employed it with success. The first of these, Dr. John Hunter, was well qualified to give an opinion on the subject. He had received a full and ample education both in medicine and surgery. He had been physician to the Fleet for many years, and had seen a great variety of climates, and at last resided in London, where he was much respected. I must here refer the reader to his essay on this fever, in the third volume of the Medical Transactions of the Royal College of Physicians; in which paper, there are also, many valuable remarks on the local origin of contagion.

There is another innovation on old practice, recommended by Dr. Clarke, which was the early employment of laudanum in large doses. He says, 'In great watchfulness I have found it to be the best plan to give forty, and sometimes fifty to sixty drops of tincture of opium to the patient at his usual hour of going to rest, and to repeat twenty-five drops more in an hour's time if necessary.' He adds, indeed, that from twenty-five to thirty drops will be sufficient to begin with. Now, it is not the employment of laudanum in the first or in any stage of continued fever which was new, when the patient was much agitated or became nervous from want of sleep; but to give large doses of it in the first stage, when in general there is a great tendency to delirium or coma, was new; and, from all that I have experienced, I should call it bad and hazardous treatment.

In the early part of my practice, I was often tempted

to give it both in moderate and full doses in the hope of procuring sleep and calming the patient; but I soon renounced it at that period of the disorder, having observed that it seldom produced any positive good, while, on the other hand, it occasioned too often positive evil.

If I except cases of ephemeral fever, opium, and its common preparation laudanum, have generally failed, in my hands, to produce a quiet refreshing sleep in the first stage of continued fever. In full doses it begat a dangerous and oppressive stupor, which unfortunately terminated at times in coma. In all cases, without almost any exception, it produced great dryness in the mouth and a suppression of secretions, without rendering any compensating benefit. In the middle stages of fever, when there was no counter-indication, it was employed more freely; and when combined with small doses of antimonial wine, and nitrous ether, it often did good. Of late I have preferred the acetate of morphia to common tinctura opii or laudanum; as it appears to me to have less heating or stimulating qualities.

As cases are more illustrative than general descriptions, and as the great object of this commentary is not only to warn young practitioners of all that I deem hazardous, but to incite those who, by their age and experience, are best fitted to give an opinion on disputable points, I shall select the first case in Dr. Clarke's work, and which I suspect to be his own, by way of exhibiting his practice in the very first and subsequent stages of a mild form of typhus:

'A gentleman, aged forty-seven, after being much 'exposed to contagion, was, on the 26th January, 1791, 'seized with chillness, headache, universal lassitude, and 'debility. At bedtime he immersed his feet in hot water, 'drank some warm gruel, and perspired in the night. 'But he had no composed sleep; found his head con-

'fused, and was constantly and suddenly awaked by 'phantoms. On the following day his headache and 'languor continued. His pulse was not much ac- 'celerated, he had no appetite, and passed the following 'night in a restless state. On Friday the 28th, during 'the whole day he was harassed with headache, chillness, 'great oppression, puffing of the stomach and bowels, and 'had a diarrhœa. He was obliged to get up during the 'night in the exercise of his profession, and, if possible, 'was resolved by every mental exertion to combat his 'complaint.

'On Saturday, the 29th, he was no better; and, being 'engaged, he sat up the whole of the night with a young 'lady in the last stage of fever.

'On Sunday morning, the 30th, he was seized with 'shivering, vomiting, and increased headache. His eyes 'now became confused and impatient of light; and he 'had such a degree of muscular debility, giddiness, and 'faintness, that he was obliged to take to his bed before 'mid-day.

'After the operation of an emetic, he took four ounces 'of the decoction of bark with tincture of colomba; and 'repeated the same quantity every hour and a half, with 'one drachm of the powder of bark in every alternate 'dose.

'31st. He passed last night in a restless state; had 'frequent twitchings and startings of the muscles in 'various parts of the body; his ideas were at times con- 'fused; and he had now and then flashes of light before 'his eyes. He, however, persevered with the decoction 'and powder of bark; and in the morning, increased the 'last to two drachms for a dose.

'February 1st. He still had a restless night; and, when- 'ever he shut his eyes, notwithstanding he had taken an

'opiate, he saw strange objects, and was often troubled 'with convulsive twitchings of the muscles of the legs, 'arms, and shoulders. His urine continued pale, but he 'persevered regularly in the use of the bark, taking a 'pint and a half of the decoction, and two ounces (!!) of 'the powder in twenty-four hours; which sat easy on his 'stomach, and produced an equal warm perspiration. At 'bedtime, after bathing his feet in warm water, he took 'sixty drops of tincture of opium.

'2nd. He rested four hours last night. In the morn- 'ing his urine was turbid. His skin soft and moist. His 'headache, giddiness, and twitching of the muscles were 'still troublesome; but he continued through the greater 'part of the day in an agreeable warm sweat. He per- 'severed in the use of the decoction, and took one-sixth 'of an ounce of finely-powdered bark every three hours. 'And, as the quickness of his pulse had now subsided 'considerably, he drank about a bottle of claret every 'twenty-four hours.

'For the three following days he had little fever; slept 'better at night, but his urine continued pale, and he was 'very feeble and oppressed, except after taking bark and 'wine, which always gave him fresh spirits. He still 'persevered in the bark, taking twelve drachms of the 'powder, and a pint and a half of the decoction every 'twenty-four hours.

'On the 5th of February, in the afternoon, though 'very weak, he ventured to be carried out in a chair to 'visit a gentleman who was taken ill of the same fever, 'and who was anxious to have his advice.

'On the 9th of February, his urine was again turbid; 'but in the afternoon it became pale. His pulse fluc- 'tuated from 90 to 100; and he was constantly low and 'oppressed, except after taking bark, wine, or nutriment.

'From this time to the 12th, he found a disposition in 'the fever to recur; notwithstanding, he still took the 'bark, to nearly the quantity of an ounce daily.

'On the 19th, 20th, and 21st days of February, his 'urine became considerably loaded, resembling milk 'chocolate; and from this time he rapidly recovered his 'strength and usual spirits.'

This case is followed by the history of ten others; some of which commenced with bilious symptoms. In the greater number, petechiæ appeared in the course of the disorder. All of them were healed in a similar manner by large doses of cinchona and opium; and all of them terminated favourably!

The observations I have to make on the first case, apply more or less to all. These remarks are few in number. They occurred to me on my first perusal of Dr. Clarke's work, and were the causes why I never could make up my mind to place implicit confidence in his authority, so as to give a fair trial to his experiments. But I shall add some facts which occurred afterwards, and which confirmed me in the belief, that Dr. Clarke's practice was both doubtful and dangerous.

In the first place, it may be remarked, that the case, judging by the commencement of the attack, and by the symptoms of the first stage, was one of the very mildest forms of contagious typhus. The affection of the brain could not have been great, since it allowed the patient, evidently a medical man, to direct the treatment, not only in the beginning, but to have controlled it throughout its whole course. It did not begin with the portentous coma or very violent delirium, which generally characterises the bad cases of typhus gravior. And here it ought to be observed, that Dr. Clarke does not insist on the employment of cinchona in large doses in those affections of the brain. He had the merit of being one

of the first to suggest the use of mercury in such affections. (See note at page 297 of his Observations, second edition.) The fever, however, mild as it was, lasted upwards of twenty-six days, under this improved method of treatment; and I confidently assert, that similar cases of typhus would have been cured much sooner by the mild antiphlogistic plan of treatment, which was general among the greater number of London practitioners, whether physicians or intelligent and experienced apothecaries.

The doctrine of Dr. John Brown, which I suspect had great influence on the minds of Dr. Clarke and some others at that time, never acquired much celebrity in Great Britain, except as an ingenious invention. The good sense and practical talent of the great bulk of British physicians, soon led them to discover its total inapplicability to such complicated diseases as fevers; in which the disordered state of each organ or function becomes an object of special and serious consideration: but it was quite different in some other parts of Europe, and more especially in the German and Italian universities and academies. The simplicity of the doctrine caused it to become a favourite one for teaching. It was easily comprehended by students, and, unfortunately, many of the learned professors who taught it, were, either men of very limited practice, or else, though full of learning, and endowed with more than enough of imagination, they were defective in that sagacity which is so necessary for the safe exercise of a difficult, imperfect, and empirical art, which depends more on the results of past experience, and careful comparisons and observations often repeated, than on theoretical deductions.

Dr. Andreas Roschlaub; Drs. A. F. Markus; Joseph Frank; Adolphe Henke; J. H. Müller; and Christopher Fr. Oberreich, taught a modified version of Brown-

onüsm, under the German title of the *erregungs theorie*, (theory of excitation,) which infected, more or less, many schools of Germany from 1798 to 1806.

In 1804, I had the honour of being invited to become physician to the Emperor Alexander; and on my arrival in St. Petersburgh, I found the *erregungs theorie* in full blossom among the younger members of the faculty, especially among those who came from German schools, a great many of whom were engaged to enter the Russian service, as the native physicians and surgeons were not at that time sufficiently numerous to supply the wants of the army, navy, and civil departments.

The whole of the civil hospitals, with their medical officers, as well as the government and district physicians and surgeons, were placed under my direction. Many of those who had been regularly taught their profession either in the Russian and German universities and academies, before these became contaminated with doctrines that led either to very hazardous or to very ineffective practice, were highly useful and meritorious men. But with the disciples of Roschlaub, Markus, and others of the excitement theory, and with those of Shelling, Killian, Oken, and other professors of the new tribe of *philosophising physicians of nature*, (die natur philosophischen aerze,) who succeeded them, and who began to invade the empire about the year 1810, I had frequent cause of dissatisfaction and pain.

In the treatment of typhoid and all kinds of adynamic fever, the practice of the gentlemen of the erregungs theorie was conformable to the worst interpretation of Brown's doctrine. I say the worst, for I wish it to be understood, that I am one of those who think that some of Brown's opinions, when rightly interpreted and modified, were useful generalisations, which have promoted the gradual improvement both of theoretical and prac-

tical medicine. But Brown himself, owing to his never having been a practical physician, did not know how to appreciate and limit his generalisations, far less to apply his truly novel and ingenious speculations to practice. He, before all others, opened the understanding of medical men to some of the most useful laws of stimulation. His doctrine of indirect debility from excess of stimulation, and of direct debility, either from the absence or the withdrawing of stimulants, though, perhaps, frequently acted on, had never before been so well generalised and so clearly expressed as a law; and yet its application to diseased actions has no exceptions. The great practical defect of his hypothesis, arose from his trying to apply the law to the whole of the human body at once, when labouring under a multitude of functional disorders at one and the same time, as in the case of fever. For in this disease, as well as in several others, some of the organs are in very opposite states of excitation, as also in different kinds and states of debility. The same degree of stimulation and the same degree of abstraction of stimulants, are necessarily not suited for all of them at the same moment. But one of the greatest evils, which the misapplication of Brown's doctrine occasioned, was the almost total neglect of the necessary evacuations, either as regarded their frequency, quantity, or quality. When a thorough-going Brownonian had once made up his mind that the general character of the disease he had to treat, was asthenic; stimulants alone were to be employed. A moderate loss of blood to relieve local congestions, an emetic or a cathartic to relieve the stomach and intestines from their noxious and irritating contents, though perfectly reconcileable to Brown's general propositions, were not commented on by himself, nor were they duly expounded or explained as essential parts of general treatment by any of his

numerous commentators whose works I am acquainted with. By all his pupils, the typhous fever, in all its varieties and modifications, was considered as a generally asthenic affection; and, consequently, the whole practice was directed to that object. Hence, opium, (as a diffusable stimulant,) musk, camphor, tincture of valerian, infusion of the same and of serpentaria, with wine and other alcoholic liquors, in gradually-increasing doses, were frequently exhibited, not only in the first, but in all the stages of the disease, without almost any regard to the cerebral, pulmonary, or abdominal affections.

The following anecdote may perhaps be thought by some of my readers not to be perfectly relevant to the historical relation of the practice I wish to describe, but it explains why in an advanced period of my life, when the generality of practical physicians abandon books and doctrines for empirical facts, I should have been forced, by unforeseen circumstances, to recommence, as it were, the study and progress of the doctrinal part of medicine, as well as to the accidental improvement of the empirical or purely practical part of it.

Many excellent institutions for the education and maintenance of the children of the poorer classes of the nobility and *bourgeoisie* of St. Petersburgh and Moscow, as well as the over-populous foundling hospitals and charities for the sick poor, were under the special care and protection of the Dowager Empress, Mary, the mother of the late and present Emperors.

The devotedness of this august, highly-principled, and excellent lady, to these numerous institutions, cannot be described so as to convey an adequate idea of the unremitting care she took of them, or the personal inconvenience to which she put herself, in order to satisfy the conscientious feelings, as to the duties they imposed on her. Scarcely a day passed, that she did not visit one or more of the

institutions; and her inspection into everything that regarded either the moral or physical welfare of their numerous inmates was most minute.

In each of them were one or more sick wards, to which were attached two or more physicians or surgeons, an apothecary, and an apothecary's shop amply furnished with the very best medicines. She visited the sick wards, as well as every other part of these mansions, and every morning received from each of the medical officers a regular and minute report of the number of the sick, their diseases, the recoveries, amendments, and deaths.

In cases of severe illness or danger, and in such a number of institutions these cases occurred frequently, I was ordered to give daily attendance, notwithstanding the number of my other duties and avocations. This was assuredly a very flattering mark of confidence, but it exposed me to more jealousies, vexation, and torment than I can express; for never before, nor since that time, have I met with young physicians so completely fascinated by hypothetical opinions, as the disciples of the *erregungs theorie*, except it be those who issued from the schools of Shelling and Killian,—I mean the philosophical or philosophising physicians of nature.

With the disciples of Brown I could reason; and I often attempted to convince them of the inapplicability of his doctrines to practice, especially in such a complicated disease as fever. But with the philosophical physicians of nature, I found, after many trials and much labour lost, that I neither understood their philosophy, their singular definitions, nor their language.

Dr. F. W. J. Schelling, the real founder of this extraordinary school, is unquestionably a man of great erudition and genius, but of too metaphysical a turn of mind for the practical application of an imperfect science. He appears to me, judging by his works and specula-

tions, to have imbibed a notion, that it was necessary to withdraw the human mind from the common and vulgar contemplation of physical phenomena and facts, and to elevate it to the contemplation of, what he deemed, a more sublime philosophy; in short, to transcendental speculations on nature.

As instances of the great discoveries which may be made by this method of philosophising, a method which despises the vulgar beaten path of slow and patient induction, and which springs at once into the most hidden arcana of nature, I shall give a few of them as specimens of the science.

'Light,' according to Schelling, 'is the separation of 'that which is absolute; it is the primitive action (uhr- 'handeln) of pure activity; it is the animating soul of 'ether, by which it is placed in polar tension.'

This definition is intended to convey a clear notion of light. Now for matter.

'Matter arises from the action of the attractive and 'repulsive forces; and both of these forces are to be 'considered as the general laws and principles of a dyna- 'mic theory.'

'Magnetism is the first and chief momentum in the 'generation of matter; it is the general form of finite 'existence passing into the infinite; it is the general 'mode of animation; the general form of individuality in 'self-existence. It is consequently the condition and 'category of matter, not exclusively of individual bodies, 'but of all bodies whatever.' And,

'Disease is a change of,' what Schelling calls, 'the 'dimensions of human *organism*,' viz. the functions of reproduction, sensibility, and irritability; but why they are called dimensions is not to be understood. Troxler, one of his disciples, defines disease to be a disproportion between organic activity and organic structure. Walther,

another learned professor of the same school, informs us that disease is the opposition of the special or specific quality of *organismus* united to the *idea* of life. And Kepler says it is 'the reigning difference of individuality 'and universality.'

Here was a perfect confusion of ideas, a kind of scientific olla podrida, from which I despaired of extracting any mental nourishment either for the benefit of my young friends or my own.

The Dowager Empress, like many ladies of rank and education, believed she knew much of medicine. Her doctrines were Boerhaavian, which made it easy for any one who was equally versed with herself in the theory of that school, to answer her numerous questions, and satisfy her mind concerning the nature of most diseases, and the reason of the treatment. But her manner of estimating the comparative talents of the physicians and surgeons who were attached to her institutions, was a very plain practical one; which, however, did not accord with the philosophy of the times; namely, the success or the evident results of their practice. It was in vain for an unsuccessful practitioner to attempt to justify his treatment, by proving that it was *secundum artem*. She had lived long enough to have witnessed in her own august family, as well as among the public, the successful treatment of diseases by many of the old court physicians, the pupils of the Dutch, Berlin, Vienna, and Edinburgh schools; and she, therefore, could not avoid comparing them with the newly-imported disciples of Brown, Roschlaub, Killian, and Oken, &c., and the consequence was, a total want of confidence in their abilities, and a great uneasiness of mind, whenever fever, or any other serious disorder prevailed among the numerous young people who were under her protection.

I was ordered to suggest a plan for the formation of

practical physicians for her institutions. I say formation in opposition to the word education; for the students, whether they had been bred up in the Russian or German schools, were duly prepared by preliminary education, not only in ancient and modern languages, but in natural history, and philosophy, in anatomy, physiology, chemistry, botany, and all the auxiliary sciences.

It may be easily conjectured that the only remedy I could invent, was the establishing, in one of her Majesty's hospitals, a practical clinical ward, under her immediate protection, in which the investigation and treatment of cases should be conducted under the daily instruction and control of experienced physicians and surgeons.

I delivered to her Majesty a *memorial* on this very important part of medical education.

The plan was approved by her Majesty; and, although I had named to her a practical physician whom I deemed perfectly qualified to be placed at the head of this kind of school, she declined the proposal, and insisted on my carrying it into execution myself.

Happily for my peace of mind, and perhaps also for my reputation, the Emperor declined to confirm the project of her Majesty.

But a sufficient interval had elapsed between the delivering in the memorial and the answer received, to afford me time for serious reflection on the task which was about to be imposed on me.

The young men, whose introduction into practice I was to direct, were duly prepared, as I have already stated, by preliminary studies, both in languages and science; but they had imbibed false doctrines of disease, which perverted their understanding, and which necessarily tended either to very dangerous errors in practice, or to a total indifference for any kind of mental employment but those of a metaphysical and most unsatisfactory character.

The critical philosophy of Immanuel Kant, the transcendent *idealism* of T. Gottlieb Fichte, the natural philosophy of F. W. Joseph Schelling, the philosophical reveries of Oken, and many others, had contaminated, as I have said, many schools of medicine in Germany at that period, as must be well known to every one who is acquainted with the medical literature of that epoch. How to bring back the minds of such young men to that state of purity, which is a preliminary condition for the admission of fair argument, grounded on processes of inductive inquiry, was a task I despaired of. It appeared to me that there was but one course to be followed with men whose minds were already strongly prejudiced in favour of particular doctrines, which were quite unfitted for practical purposes. This was to convince them, by daily experience, that in the safe practice of so imperfect and difficult an art as medicine, recourse must be had at all times to the results of rational empiricism, as an useful test of our doctrines, and that any doctrines which stood in opposition to such general results ought to be rejected as a fallacious guide. But being satisfied, at the same time, that without some theory to connect the various facts which belong to the history and treatment of disease, I should make very little lasting impression on the minds of my scholars, I was forced to re-examine my old manuscripts and course of lectures, which I had happily carried with me; and, consequently, to re-consider many doctrinal points, but especially those which had an influence on practice in fever. The hypothesis with which this commentary concludes, will serve as an example of the manner in which I think the doctrinal part of this disease ought to be studied.

With what success I should have accomplished the task, I cannot presume to guess; but if I know myself at all, I am sure I should have exerted my best endea-

vour to have formed a few useful practical men according to the notions I entertain of practical medicine; comprehending such explanations of disease as a sound experimental physiology justifies, and causing the student to try all doctrines by a reference to the results of experience.

For the present, I return to my narrative of the new and doubtful doctrines and dangerous innovations in practice, which have succeeded each other so rapidly in the present century.

I pass over the doctrine of Schelling and the philosophical school with the few remarks I have already made on it, as it did not lead to any new practice, or, in fact, to any practice whatever, as will be best understood from an answer I received from one of my young friends, when questioning him how he would act in a particular stage and state of fever which I described to him. 'Sir,' (he said,) 'we are taught to think deeply and to write on 'the philosophy of nature and of medicine, but not to 'practise it. Procure a professor's chair for me; that is 'my vocation.'

Of Rasori's Brownonian practice in 1799; *of his sudden abandonment of the same; and of his invention of the doctrine of Counter-irritation, with exemplications of practice.*

Towards the end of the year 1799, after the seizure and occupation of the Ligurian territory by the French army, a highly-contagious fever began to appear, first at Nizza, and afterwards in Genoa, in which unfortunate city, as we are informed, fourteen thousand six hundred persons died of it in the course of six months. It assumed the character of a putrid or petechial fever. Dr.

Brignole, in his description of this dreadful epidemic, mentions, as an effect of the pernicious malaria which seems to have been engendered by the number of the sick, that many people were so debilitated previously to any attack of the fever, as to be disposed to gangrene, and that several were disfigured by the loss of their noses by this accident. He also mentions another fact of considerable interest in a practical point of view, which corroborates an observation of Dr. Robert Jackson and of some German military physicians, who afterwards had frequent opportunities of witnessing its truth; namely, that the patients who were the most neglected by the medical officers, and who had scarcely any shelter either from rain or vicissitudes of atmosphere, were those among whom the mortality was the least. This was particularly remarked in that part of the city destined for galley criminals, who, it would appear, had been happily neglected as well as condemned; for when we turn our attention to the treatment of the fever in the great military hospital after Rasori assumed the superintendence of it, we shall have reason to think that the condemned criminals were not the most unfortunate.

Giovanni Rasori, after having finished his literary and medical education at Pavia, travelled into other countries to enlarge his knowledge; and, among others he visited Scotland, where he became acquainted with the doctrines of Dr. John Brown, of which he became an admirer and zealous propagator; for after his return to Pavia, where he was appointed professor of pathology, we find him translating the works of that author, and teaching it in his lectures.

The practice of Rasori in typhous fever at that period was strictly consonant to the principles of Brown, as they were understood by him and many others. It consisted in giving five or six grains of solid opium daily, in divided

doses, together with powerful stimulants, both of the diffusible and permanent kind.

As an illustration of the practice and of the judgment of the physician who afterwards acquired so great a fame among his countrymen, on account of his doctrine of counter-irritation, I shall select one case from the reports of his clinical hospital, as they are published by his pupil Ozanum.

John F. Forestier, by birth a Frenchman, but an inhabitant of Milan, forty years of age, by profession a house carpenter, a man of very robust constitution, was seized, while in Brescia, with a quartan ague in January 1811. He was admitted into the hospital, and was cured of his ague by means of cinchona. Soon afterwards he walked by forced marches from Mantua to Milan. Scarcely was he arrived in the last-mentioned city, when, being very thirsty, greatly fatigued with his journey, and perspiring profusely, he drank a large quantity of cold milk. On the same day his fever returned. He was immediately admitted into the town hospital, but being selected as a proper patient for Rasori's clinical institution, he was afterwards transferred to it.

On the first day after his admission, he was ordered five grains of opium. On the second day six grains of the same and a pint of wine; after which he was seized with delirium and a convulsive hiccup. These symptoms continued the two following days; when the fever became a continued one, which was characterised by a very distressing cough, violent pain in the epigastrium and the whole region of the chest, with bloody expectoration. He was then ordered emetic tartar in large doses, and kermes with lemonade. He was also blooded copiously *fourteen times*, but instead of getting better, he felt himself getting worse; when on the fifth of June, becoming impatient and discontented,

he took French leave both of his doctor and the hospital. He continued to suffer from the cough and dyspnœa, became rapidly emaciated, and so weak that he could not work. He dragged on a miserable existence until the twenty-fifth of October, on which day, after a violent paroxysm of coughing, he was seized with a profuse spitting of blood. He was again bled at his own lodgings, and again brought back to the clinical hospital; where he died of purulent phthisis on the twenty-fifth of February following.

A short time after the fever had broken out in Genoa, of which mention has been made, Rasori was ordered to proceed to that city and to assume the superintendence of the military hospital there. Here he had abundant scope to try his method of treatment on a large scale. At first he continued faithful to the Brownonian practice, employing diffusible and permanent stimuli with tolerable freedom. But the mortality increased rapidly. Being astonished, I suppose, that a doctrine, which to him appeared to be true in its principles, should fail in its application to practice, he seems to have accused himself of too much timidity; and, therefore, he doubled his doses. But, as Eble says, death followed his footsteps closely, and at last he became convinced of the danger of this mode of treatment.

Some cases of the disease having commenced with violent delirium, Rasori was induced to try venesection. The symptoms were mitigated, and this success encouraged him; he then took away blood more freely, and still with happy effects. At last he began to bleed very largely; after which, he gravely asserts, he did not lose a single patient.

New ideas presented themselves to the active and vivid mind of Rasori, which, of course, were not very clear at first, nor indeed at any time afterwards, as we shall

soon see. But, instead of betaking himself to the study of old and approved practical authorities, he began to read and to be captivated with Darwin's Zoonomia, and out of this author's vagaries, and those of Brown, he concocted his doctrine of counter-irritation.

This doctrine, which bears the same title with that of a work published in this country by Dr. Granville, and also with an excellent little essay, under a similar title, in that useful book of reference, the *Cyclopædia of Practical Medicine*, has no resemblance to either of these works, except it be in name.

According to the Italian doctrine, the activity of the living principle is either exalted, and the organic fibre is in a state of tension and contraction; or else, the activity of the living principle is depressed and lowered, and the organic fibre is in a state of relaxation.

The first of these conditions is called the *diathesis di stimulo*, the other the *diathesis di contra-stimulo*.

The signs of the first diathesis, viz., that of irritation or stimulation in muscular parts, are, an increase of power, a disposition to cramp, (*convulsibility!*) and in the nervous system, a wandering of speech, and many other nervous affections; and in the vascular system, strong arterial pulsation, and palpitation of the heart! and in the lymphatic and exhalent vessels, an increase of excretions and of all the functions, also diarrhœa, diabetes, and many other very odd classifications of diseases.

The signs of the diathesis of counter-stimulation in the muscles, are, a low degree of irritability, of muscular firmness and solidity, or little disposition to contractility; and in the nervous system, somnolency and deficient intellectual energy, low delirium, lipothymia, and singing in the ears; in the vascular system, the symptoms are a weak small pulse, and feeble action of the heart; the functions of the stomach and intestines are weak and

deranged. All external agents and medicaments are said to act either as stimulants or counter-stimulants. And it is asserted that this law holds good also in regard to the action of the different organs and organic elements of the body on each other. For instance, the blood acts as a powerful and principal stimulant, but all the other fluids are to be considered as counter-stimulants. There are, in fact, only too classes of agents in nature, which affect life, viz. stimulants and counter-stimulants.

The first diathesis is said to be more general than the second! Brown's notion that asthenic diseases are the most common, is a mistake according to Rasori, and arises from the application of the untenable doctrine of indirect debility!

The following assertions are still more extraordinary: all diseases which belong to the first diathesis, namely, that of stimulation or excitement, are said to terminate naturally in health! or, at all events, to require little medical assistance! whereas, those which spring from the second diathesis, if left to themselves generally end in death. Nevertheless, it will be scarcely credited, after such an assertion, that, according to this doctrine, all chronic disorders, such as scrofula, syphilis, arthritis, and even cancer, are supposed to arise from the first diathesis.

As symptoms are confessed to be very deceitful witnesses, when it is required to discover to which diathesis any given disease belongs, recourse must be had to a more certain and useful way of ascertaining this important point of practice, and that is, by the means of the action or operation of medicines!

But in order to ascertain the value of each medicament, we must first discover one which shall serve as an object of comparison for all others; and that remedy is

said to be blood-letting; for its effects are not only certain, but are generally understood and acknowledged. Blood-letting is, therefore, pronounced to be a *medicamentum tertii comparationis gradûs.* The action of all other mendicaments is either more or less analogous, or more or less opposed to that of the loss of blood. Hence it follows, that all remedies fall under two classes; one of which is adapted for diseases of the first diathesis, and the other for those of the second. A small blood-lettting, as a proof-trial, will enable any one to determine what diathesis prevails in any doubtful case. But after a sufficient number of careful comparisons have been made and collected, relative to the operation of medicines, the necessity of a preliminary blood-letting, as a criterion of the diathesis, will become daily less necessary; and, therefore, this test, which, it is admitted, may be a dangerous one to institute in many cases, will only be resorted to in cases of great uncertainty.

After arriving at this comforting conclusion, Rasori, who until then had taught the doctrines of Brown, now declared his opinion, that indirect debility from excess of stimulation was false. According to the new lights of the *Nuova Dottrina vera Italiana*, the diseases which, until then, were supposed by him to arise from indirect asthenia, were now considered as belonging to the diathesis di stimulo, and consequently to require counter-stimulants for their cure.

The confusion of ideas which characterise the pathological reasoning of this school, is to be observed in the classification of diseases, where some of the most opposite in their nature are brought together under one category, and, of course, are recommended to be treated on one general principle. Thus, almost all the exanthemata, with or without fever, gout, anasarca, ophthalmia, pneumonia, typhus, and the diarrhœa which follows

measles, are pronounced to belong to the first diathesis, and must be treated by blood-letting, emetics, and other powerful counter-stimulants.

To the class of stimulants belong, ammonia, opium, musk, camphor, carbonic acid! alcohol, and all spirituous and vinous liquors; the etherial essential oils, especially oil of cloves, of cinnamon, and pimento; also ethers, caloric, and the blood and muscular fibre of red-blooded animals. While, on the other hand, counter-stimulants are the various fluids and fibres of white-blooded animals, prussic acid, arsenic! laurel water, Kermes mineral, tartar emetic, and all antimonials; mercury, and all mercurials; all bitter remedies, acids, digitalis, belladonna, conium, aconitum, gum gamboge, hyosciamus, &c. And by way of summing up, we are gravely told that all counter-stimulants belong exclusively to the mineral kingdom; while several direct stimulants are derived from the vegetable one, and many more from the animal kingdom.

I must be pardoned if I do not dwell longer on this doctrine. Its practical effects in the treatment of typhoid fever, was the employment of blood-letting, and the exhibition of digitalis to such excess as had never been known before. Tommasini, and others of the school, take occasion to eulogise it, as having prepared the *understanding* for the practice of Broussais, Armstrong, Clutterbuck, and others, which soon followed this period; and which consisted in taking away large quantities of blood in the early and even in the second and later stages of typhoid fevers.

Whoever will take the trouble of reading with attention, and with a view to the results of experience, the many valuable essays and works which have been published on the subject of continued fevers, from the time of Sydenham to the year 1780, will find that, with very few exceptions,

the authors of these works recommend the abstaining from copious blood-letting in every stage of typhus, not from preconceived or prevailing hypotheses concerning its asthenic character, for many authors believed typhus to be inflammatory in its first stage, but solely because experience and attentive observation had taught them that the whole powers of life sink suddenly and very unexpectedly if more than ten or twelve ounces of blood are withdrawn at a time in this disease.

So slightly does vitality seem to adhere to the human frame during the disorganising process of adynamic fevers, especially in the second and later stages, that the injudicious exhibition of a common purgative remedy often produces a collapse, out of which the system is with difficulty roused, and I have often seen the abstraction of ten ounces of blood produce a fatal result.

It ought not to be taken for granted, in consequence of merely gratuitous assertions, that the continued fevers of the present century are specifically different from those which prevailed every year in Europe for several centuries before, and, consequently, that they require a totally different mode of treatment.

Admitting, as every old practical physician must do, that the European continued fevers are much diversified at times by atmospherical causes, and that in so variable a climate as that of Great Britain we may have in the spring of one year a typhous fever, accompanied by catarrhal symptoms, while at a later season of the same year, according to the heat of the summer and the moisture and variable temperature of the autumn, the typhus may be complicated either with bilious symptoms or with petechiæ, nevertheless the pathognomic character of the fever is essentially the same; that is, a fever with great prostration both of muscular strength and nervous energy from the very beginning to the end of the disease.

In fevers which accompany active inflammation, such as carditis, peritonitis, enteritis, &c., there is indeed, from the beginning to the end of the disorder, more or less inability to exercise muscular motion, and consequently an apparent debility; but in no febrile disorder, except typhus and its varieties, is the energy of the brain so pre-eminently enfeebled by the exciting causes of the complaint on the very first attack; and it is for this reason, that I always taught in my lectures, that when a fever of a doubtful character presents itself with catarrhal symptoms and pleuritic pains, the state of the intellectual faculties is one of the best means of judging of its true nature; for whenever these are debilitated and confused in the very onset of the malady, typhus is to be suspected. But, to return to the dangerous innovation in practice which was now about to be introduced by the authors just named, I trust it will not be deemed out of place, to adduce the opinions of Sydenham, and also of some other eminent practical physicians, on the subject of blood-letting in continued fevers; after which I shall endeavour to show how the undeniable fact of local inflammation in the viscera of typhoid patients, is to be reconciled with the results of old experience.

Sydenham, in describing his treatment of the fevers which raged in London in the years 1661, 1662, 1663, and 1664, necessarily takes notice of venesection, and expresses himself thus concerning it.

'Quoties mihi cum ægris res est, quorum sanguis vel 'per se imbecillior existit, uti fere in pueris,' (which, by the way, is a singular assumption on the part of Sydenham,) 'vel justâ spirituum copiâ destituitur, ut in de-'cliviore ætate, atque etiam in juvenibus diuturno aliquo 'morbo confectis a venæsectione manum tempero. Enim, 'vero si phlebotomiam his imperarem, sanguis etiamnum 'citra ejusmodi imminutionem plus satis debilis, des-

'pumationi suæ obeundæ prorsus impar redderetur; *unde* 'totius massæ perversio ac proinde forsan ipsius ægri inte- 'ritus sequeretur.'

Large blood-lettings in the continued fevers of Europe, especially those which are contagious and of a typhoid character, were never recommended by Boërhaave, Van Swieten, or any of their school, neither by any of the British physicians, such as Mead, Huxham, Sir John Pringle, Cullen, Sir Gilbert Blane, &c., until the beginning of the present century; and indeed they almost all insist on the necessity of great caution in the use of the lancet. One of the most distinguished physicians among the Germans, Dr. Samuel G. Vogel, who practised at Steudal in 1785, in his Manual of Practical Medicine, (Handbuch der Pracktischen Arzneiwissenschaft,) has recorded the result of his experience concerning phlebotomy in typhus, in the following words: (vol. ii. p. 30:) 'In no fever whatever is blood-letting so seldom admis- 'sible as in this, except in very rare cases, and then only 'in the commencement of the disease, and in small 'quantity. It does happen, indeed, now and then, that 'the fever begins with inflammatory symptoms, and that 'a patient can bear the loss of some blood; but let the 'physician take heed lest he allows himself to be de- 'ceived by false appearances or by the opinions of 'patients who expect to derive relief from the loss of 'blood; for if he does bleed, he may commit great in- 'jury which cannot be afterwards repaired.'

But Sir John Pringle, whose works on the diseases of the army cannot be too attentively read or too accurately remembered, expresses himself more strongly and less hypothetically than Sydenham, on the subject of blood-letting in the first stage of typhus. He says,

'When the symptoms run high, a plentiful evacuation 'of this kind (of blood) seemed indicated; yet I observed

'that large bleedings generally proved fatal, by sinking 'the pulse, and bringing on delirium. Nor was a mo- 'derate bleeding to be repeated without caution. Even 'those,' (he adds,) 'whose blood was sizy, unless the lungs 'were inflamed, were the worse for a second bleeding. 'If the head only suffered, I judged it safer to bleed by 'leeches applied to the temples, than to open a vein in 'the arm.'

As a perfect contrast to such instances of practical wisdom and sagacity, I select another case from the reports of the clinical hospital of Rasori, after the concoction of his new doctrine.

'A man of robust constitution was admitted into the 'clinical institution on the second day of his disease. 'He had considerable fever and severe pain on the right 'side of the chest. His pulse was strong, and his breath- 'ing short. He was ordered to lose two pounds of blood 'at once, and to take thirteen grains of digitalis.

'On the third day, the pain continuing, and the pulse 'being 112 per minute, a second blood-letting, to the 'amount of eighteen ounces, was ordered; together with 'twenty-four grains of digitalis, which he was to take in 'the course of twenty-four hours. He was also ordered 'to lose eighteen ounces more blood in the evening of the 'same day.

'On the fourth day, as the symptoms were not re- 'lieved, two more venesections, each to the amount of 'eighteen ounces of blood, were ordered, one for the 'morning, and the other for the evening; and the dose 'of digitalis was increased to thirty-six grains.

'On the fifth day two blood-lettings to the same amount 'and two scruples of digitalis.

'On the sixth day every symptom was worse, the pulse 'was small, and beat 108 per minute. Great prostration 'of strength; nevertheless, the patient was ordered to lose

'another pound of blood, and to take two scruples of 'digitalis.

'On the seventh day of the disease, there was no 'amendment; and he was again ordered to be blooded, 'and to take the same quantity of digitalis.

'On the eighth and ninth days, the breathing was very 'short; the pulse was very small and frequent; and the 'patient was worse in every respect. One pound of blood 'was withdrawn, soon after which, the breathing was ac-'companied with a rattling noise in the trachea, and 'great anxiety, which terminated in death.'

During his illness, the patient had lost fifteen pounds of blood, and swallowed two hundred and twenty grains of digitalis! It will, I think, be admitted that this patient must indeed have been a very strong man, as described in the case.

The doctrine of counter-irritation rapidly acquired great celebrity in Italy, under the pompous name of the Nuova Dottrina Italiana, which Tommasini, the most distinguished of Rasori's pupils, in a rapture of praise, calls '*una vera figlia del Solidismo e del Brownianism*,' which I presume is meant to convey a clear notion of the two pillars on which it is founded; but what the distinction between Solidismo and Brownianismo is, I do not pretend to understand.

Tommasini assures us that the doctrine was favourably received, and indeed that it became general all over Italy. He names three hundred distinguished Italian physicians who were its advocates and adherents. Among them we find Borda of Pavia, Fonzago and Brera of Padua, Rubini of Parma, Lanza in Mantua, Acerbi and Bondioli in Milan, Postiglione and Gaimari in Naples.

When it is considered that several of its devoted adherents became professors of practical medicine and clinical institutions in the Italian universities and aca-

demies, and when we also reflect how greatly disposed the minds of young men are to embrace novel opinions, solely because they are opposed to old authority, we need not wonder at its rapid dissemination. It appears, however, that the three hundred promoters of the doctrine were not all orthodox; for Tommasini admits that many of them adopted the opinions in part only, rejecting many others.

According to Tommasini and the chief supporters of this Nuova Dottrina Medica Italiana, all inflammations, without exception, are to be regarded as sthenic affections; for it is asserted that there is no such thing as asthenic or passive inflammation. It follows, of course, that in all cases in which local inflammation is detected, that the fever which accompanies it is also to be considered as of a sthenic character; consequently, typhus, the yellow fever, puerperal fever, and hectic fever, belong to the diathesis di stimulo, and must be treated accordingly. But, owing to an ingenious classification of the materia medica agreeably to the doctrine, there is, as it were, a loophole, by which the old practice in such inflammations may be reconciled to the principles of the Nuova Dottrina Medica Italiana; for it is said that bitters and metals are counter-stimulants. Hence, in *glandular diseases,* and syphilis, chlorosis! hypochondriasis, and flatulency, all of which are said to be of a sthenic character, and to arise from a similar cause, namely, the inflammation of the inner tunic of blood-vessels and lymphatics, are therefore to be treated with such counter-stimulants as steel, bitters, and squills, but naphtha is to be avoided. At the risk of being accused of a want of courtesy, I must decline further intercourse with this legitimate daughter of Solidismo and Brownianism, and leave her to her Italian lovers.

Her charms, indeed, have never been acknowledged by

any other European suitors. But in justice to the physicians of Italy in general, I must remark that Rasori's doctrines were strenuously opposed by many of them, among whom may be named, Agoni, Amoretti of Turin, Ozanum of Milan, Frederigo of Venice, Bianchi of Pisa, Gerani of Genoa, Ruffini of Modena, and many others.

CONTINUATION OF THE INQUIRY.

On the appearances of inflammation, pustulation, and ulceration of the intestines, and other viscera, which have been detected in the bodies of those who die of typhoid fever; and of the innovations in practice which arose in consequence of such discoveries.

A minute inspection of the bodies of those who have died in the second and more advanced stages of typhoid fever, has detected, in a very great number of them, unequivocal appearances of inflammation, and its consequences, in various viscera which are essential to life.

It is almost needless to state that these discoveries, which are comparatively of recent date, had great influence both on the doctrine and treatment of fevers. They induced many physicians of eminence, especially in France, to conclude that all such diseases, even the contagious typhus, were only symptomatic of the local lesions, and that there was no such disease as a truly idiopathic fever, or, to use the expression of Broussais and some others, no such thing as a *fievre essentielle.* As to the treatment, although they did not introduce the practice of copious blood-letting frequently repeated— for Rasori has assuredly a just claim to priority in realising old Sangrado's practice — they greatly encouraged and confirmed it, by showing how reasonable it was, considering the evidence of visceral inflammation.

Monsieur Andral, whose name cannot be pronounced by medical men without respect, on account of his numerous and valuable contributions to practical pathology, also on account of the accuracy of his descriptions, and, above all, on account of the candour with which he relates his unsuccessful trials, informs us that he comprehends under the general title of '*maladies pyretiques du tube digestif*,' all the fevers which, until the time of Broussais, had been classed by Pinel, as either inflammatory, bilious, mucous, adynamic, or ataxic; and which to those who still adhere to the old school of nosologists, mean all the well-known varieties of synochus and typhus.

But M. Andral is not the first of his countrymen who attempted to trace all the fevers which were formerly considered as idiopathic disorders to local causes of an inflammatory kind. M. Broussais had preceded him; and Messieurs Petit and Serres, who had also previously observed organic lesions in the intestines of those who died from fever, had classed them all under the name of '*fievres entero-mesenteriques*;' and M. Bretonneau, who had discovered a kind of pustulation of the mucous follicles of the small intestines of patients who died of adynamic fevers, reduces them under the pleasing sounding appellation of *Dothinenterite*.

Messieurs Louis and Curveillier are nearly of the same opinion; ascribing the developement of fever to local inflammatory affections of the intestines and other viscera.

It is melancholy to think that the discovery of such important anatomical facts should have tended to injure rather than to improve practical medicine; but this must always be the case when morbid appearances are erroneously interpreted; which is strongly exemplified in the present case.

To deny the existence of visceral inflammation, or its

consequences in fevers, is impossible. The facts which prove it to be true are numerous and incontestible; but as they are by no means general, this circumstance alone ought to have awakened the attention of physicians to the impropriety of considering them as the primary cause of the febrile phenomena. There is no proof of their existence prior to the horipilatio which generally announces the invasion of the disease, and we know for a certainty that visceral inflammation is not always present in typhus.

On perusing with attention the hundred and thirty-four cases published by M. Andral in the third volume of his 'Médecine Clinique,' we find that in several of those who died with every diagnostic mark of typhus, the viscera were apparently in a natural state; or, as he expresses himself, 'there was not in them *an appreciable sign of lesion.*' M. Bouillard has cited similar cases; see his *Traité de Fièvres.*] In cases twenty-four, twenty-six, twenty-seven, twenty-eight, and twenty-nine, of that author, there was no diseased structure to be discovered; and M. Louis has furnished us with observations of the same kind.

These observations are important when we reflect that M. Laennec asserts that one of the most constant results which auscultation has furnished, is the presence of a catarrhal affection during the whole course of a continued fever.

Dr. Robert Little of Belfast, in a valuable practical essay on typhus, published in the seventeenth volume of the Dublin Journal of Medicine, informs us that after the most careful inspection of the bodies of those who died of contagious typhus, during a period of four years, in the Belfast Hospital, he found himself authorised to assert that the disorder did not arise from local inflammatory action of the viscera. Out of sixty fatal cases

which he examined with the greatest care, he did not find one with signs of acute inflammation in the mucous membrane of the stomach or intestines; and in two cases only did he discover signs of inflammation of the brain. The mucous lining of the lungs was considerably inflamed in four cases out of sixty; but he did not find, in one of these sixty cases, inflammation of the substance of the lungs or of the liver; and he further states that in many cases of the fever which he examined after death, and which had been characterised by symptoms that are said to indicate inflammation of the brain, he found that organ in a natural state.

The testimony on both sides is evidently most contradictory; and we are, therefore, led to examine its validity by means of other arguments.

Admitting the fact, which indeed cannot be denied, that visceral inflammation is often, nay, I shall say generally present in the second and subsequent stages of typhoid fevers, the great questions which arise out of it, are, *first*, as to the true character of this inflammation; and, *secondly*, as to the treatment it requires.

Here, as in all similar questions of difficult solution, we must begin by collecting every fact which throws the smallest light, either directly or by analogy, on the subject.

In the first place, supposing the case to be as represented by M. Andral, that in ninety-eight cases out of every hundred of typhoid fever, visceral inflammation, pustulation, or erythema, are to be discovered, we must presume that the same lesions always existed in similar cases of fever, although the discovery had not been made before; and experience having taught us that until the time of Rasori, Broussais, Andral, and Dr. Armstrong, &c., blood-letting to so great a degree as they recommended and practised, had never been employed; on the

contrary, that it had been avoided as a very hazardous remedy; and, moreover, having been taught by experience that very many of the worst cases of typhus have been successfully cured in the advanced stages, (when ulceration and pustulation of the intestines may have been presumed to be present,) by cordials, stimulating and tonic remedies, it seems wonderful that such recollections and reflections should not have opened the understanding to the true character of the visceral affections.

We learn from the observations and cases of M. Andral, that copious blood-letting, and the means which generally cure acute enteritis, peritonitis, or pleuritis, not only failed to do good in typhus, but frequently aggravated all the bad symptoms, hurrying the patient to the grave; nay, he has furnished us with some very remarkable cases, by which it appears, that after an aggravation of all the bad symptoms had occurred in consequence of venesection, a sudden amendment took place on suddenly changing the treatment and substituting cinchona and other tonics for the debilitating agents. If, therefore, intestinal inflammation, pustulation, ulceration, or erythema, be almost always present, after the third or fourth day of adynamic fever, it is a certain fact, that they have been cured by a tonic plan of treatment, and aggravated by copious blood-letting. It is impossible to escape from such a conclusion.

But let us now see what light is thrown on these appearances, by a study of such analogous diseases as can reasonably be considered to be nearly similar, and in which there is no doubt as to the presence of inflammation and ulceration of the intestines, nor any doubt as to the right mode of treatment.

There is a well-known febrile disease which at times rages like a dreadful epidemic in hot and humid seasons, and in marshy regions. In this fever the whole alimentary

canal, from the mouth to the anus, is in an inflamed and pustulated state ; I mean the malignant thrush fever. Twice only have I seen it rage as a prevailing epidemic in England. In Holland, and some parts of Brabant, it is more common. It begins, like a regular typhus, with more or less of bilious symptoms, and about the fourth day, sometimes sooner, often later, the thrush appears. The whole mucous membrane of the mouth is inflamed ; and in bad cases the fauces become gangrenous and slough. Pains in the abdomen, with vomiting and frequent stools, in which the appearances of aphthous crusts are to be detected, follow ; and in very severe cases aphthous ulceration appears at the verge of the anus. The pulse seldom beats less frequently in adults than 120 per minute. The sensorium is variously affected in different cases, as in typhus. The disease continues from eleven days to nearly three weeks, when not fatal ; and terminates, like most varieties of typhus, by a slow recovery. In no case that I ever saw, did blood-letting appear to be necessary. Indeed, it was never dreamt of by any of my medical acquaintance, nor do I know of any author who advises it. Therefore, I cannot say what its effects may have been ; but success followed a very different, and yet a very simple method of treatment, namely, the deterging the intestinal tract chiefly by a combination of antimonials and mild purgatives, and by supporting the strength of the patient, as much as the heat, headache, and general febrile symptoms permitted, by mild nutritious drinks, such as barley-water, weak chicken broth, or weak beef tea, then by wine, and afterwards by a decoction of cinchona, combined with muriatic acid in small quantities.

Analogy also teaches us that in typhus gravior, extensive inflammation, terminating in gangrene, frequently attacks the external as well as the internal parts of the

body. When this occurs in the feet, face, hands, or nates of patients, recourse is had by almost all practitioners to wine, tonics, and cordials, as the surest means of arresting the progress of the symptoms. A mere difference of locality, however, does not constitute an essential difference in the nature of the inflammation. If it be of a passive or atonic kind, when it attacks the nose, cheeks, nates, or the extremities of patients labouring under the middle and advanced stages of typhous fever, it is not reasonable to suppose that its character is changed because it occurs in internal parts.

Analogy, when further referred to as a source of useful information, which it is the duty of every physician to consult in cases of doubt and difficulty, enables us to come to a right conclusion concerning the combination of a truly contagious and adynamic fever with inflammation; and this process of the understanding naturally directs our attention to a febrile disease which is common enough all over Europe, and in which experience alone has led to a right method of treatment, independently of all hypothetical reasoning; I allude to the scarlatina anginosa gravior.

In this disease the membranes of the mouth, fauces, nose, œsophagus, and meatus auditorius, are in a state of high inflammation. As most of these parts are objects of sight, we are enabled to discover that the inflammation is one of the earliest symptoms. The contagious poison does not seem to affect the brain sooner than it does the membrane through which it is imbibed. In bad cases it proceeds rapidly to ulceration, gangrene, and sloughing of the palate and tonsils, and to the effusion of a purulent-looking mucus from the nose and meatus auditorius. But does it enter into the head of any good practitioner to attempt the cure of this inflammatory action by taking away forty ounces or even five ounces

of blood from the arm? or to apply leeches to the neck and throat to relieve the swelling? On the contrary, does not multiplied experience teach us that this kind of inflammation is most successfully arrested by supporting the strength of the patient by means of wine, mineral acids, quinine, nutritious drinks, and the giving the patient as much drink and easily-digestible food as he can take? Therefore, we have a right to conclude that the inflammation and ulceration of the intestines and other viscera, which have been detected in the bodies of those who have died of typhus, are truly of a passive or asthenic kind, and ought to have been treated on this principle.

The first eight cases with which the third volume of the Médecine Clinique opens, all of which terminated in death, afforded M. Andral an opportunity of detecting in almost all of them a pustular or exanthematous affection of the mucous follicles of the lower portion of the small intestines. But as the death of these patients did not happen earlier than the ninth day of the disease, and in the greater number after the fourteenth day, it is too bold an assumption to say, that these local lesions were the causes of the disorder. Nevertheless, that such must have been the opinion of M. Andral and his assistants, is evident from their observations, and more especially from their practice; for blood was repeatedly withdrawn in almost all of them, either from the arm or the foot by the lancet, or by applying leeches to the hæmorrhoidal veins, to the neck, and other parts of the body.

In the greater number of these cases, delirium or confusion of the intellectual faculties, together with prostration of muscular strength, followed the blood-letting; or else these symptoms were aggravated by it. In the first case, which is one of a common bilious fever, but which was treated as an inflammatory disease, deli-

rium followed the second venesection of twelve ounces of blood from the arm. But this case is altogether so remarkable an instance of injurious practice from preconceived opinions, that I deem it necessary to translate it fully.

'A workman, (un manœuvre,) twenty-seven years old, 'had experienced for some time past a diminution of 'appetite, headache, and a feeling of unusual weakness. 'One evening after supper, he was seized with a shivering fit, and had a burning heat during the whole night. 'The following day the heat continued; he had a slight 'sore-throat, and he vomited the remains of his supper. 'In the evening of the second day he was admitted into 'the Charité, and presented the following appearances.

'The face was yellow, with redness over the cheek-'bones, (rougeur de pommettes,) supra-orbital headache, 'the blood-vessels of the eyes injected, general oppression, movements of the body painful, pains in the 'joints, the lips dry, the tongue covered with a thick 'yellow coat, but its edges and apex of a cherry red, 'a bitter taste in the mouth, little thirst, deglutition 'slightly painful, the abdomen soft and indolent all over, 'a little cough, pulse frequent and hard, the skin dry and 'having an acrid heat.

'*Twelve ounces of blood to be taken away; to drink 'barley-water, to have a glyster, and to be put on diet.*

'The next day, being the fourth of the disease, there 'was no alteration in the state of the patient.

'A second blood-letting was ordered.

'A delirium ensued on the night between the fourth 'and fifth day.'

Now, I do not presume to assert that the loss of twenty-four ounces of blood caused the delirium, although it ensued almost immediately afterwards; and although experience teaches us that delirium arises as often, nay,

much oftener, from debility and weakness, than from plethora and congestion. At all events, the loss of twenty-four ounces of blood did not prevent the symptom, which it would probably have done, if the delirium had been the effect of acute inflammation. In my opinion, delirium in this case was symptomatic of the congestive state of the liver and of the accumulation of vitiated bile.

'During the fifth and sixth days the yellow colour of 'skin became gradually deeper, (se prononça de plus en 'plus,) the patient looked at the bystanders with an air 'of surprise, his answers were slow, the yellow crust on 'the tongue was thicker. We were struck with the 'manner of his respiration; it is strong and accelerated; 'we put our ears to the chest, (nous ecoutons la poitrine,) 'and we hear a crepitant noise distinctly pronounced in 'the whole inferior lobe of the left lungs, and in the same 'a slight diminution of *sonorité des parois thoraciques;* 'no expectoration, the pulse extremely quick. The 'existence of pneumonia appears evident to us.

'A third blood-letting, to the amount of twelve ounces, 'is ordered, together with sinapisms to the inferior ex- 'tremities. The blood which was withdrawn did not 'show more coagulum than that of the two first vene- 'sections. The patient became delirious in the night.' It is useless to continue the history. He died on the ninth day.

'On inspecting the body, the inferior lobe of the left 'lung was found to be hepatized; the heart contained in 'the right side a large clot of dark-coloured, slightly 'coagulated blood; and the small intestines, to the extent 'of about two feet immediately above the ilio-cœcal 'valve, were studded with five large oval patches of a 'greyish red colour. Between the patches small pustules, 'some of which were red, others of a greyish-white 'colour, were detected; but the rest of the mucous mem-

'brane which occupied the intestines was pale. The 'whole internal surface of the large intestines was also 'pale.'

Now, I ask, if any of these appearances point out the presence of active inflammation? Daily experience teaches us, that acute abdominal tormina, such as affected this patient, and which misled the practitioner, often arise from an accumulation of vitiated bile, and from its having found its way into the stomach and intestines; and the yellow colour of the patient's skin, and the bile which he vomited, and the general character of the fever, render it presumable that this was the true cause of the pain and of the general disorder.

In the third volume of M. Andral's work, we meet again with another case of a common bilious synochus, which, after death and the dissection of the body, is pronounced by him to be one of *dothen-enterite très bien caracterisé*. On this account, and also to afford me an opportunity of adding some practical observations, I translate the chief part of it.

'A brushmaker, aged seventeen and a half years, of a 'lymphatico-sanguine temperament, had always enjoyed 'good health. On the thirteenth of October, 1822, at 'seven o'clock in the evening, he was seized, without any 'known cause, with giddiness and a sense of general in-'disposition. During the whole night he had a burning 'heat. On the following day, the fourteenth, he was in 'the same state, and he suffered from anorexia; he had 'one stool, and perspired abundantly during the night. 'On the fifteenth, he was admitted into the Charité. In 'the night between the fifteenth and sixteenth, he again 'sweated.' (I may here remark, that this is not an uncommon symptom in the beginning of a bilious synochus, though it is a rare one in true typhus with bilious symptoms.)

When visited for the first time on the sixteenth, his case was as follows: 'His face was red, and his eyes 'glistening; he felt as if bruised in all his limbs; the 'tongue was coated with a thick yellow covering, the 'lips were red, he had a bad taste in his mouth, anorexia, 'little thirst, his abdomen was soft and indolent, no stool 'for the last twenty-four hours, his pulse was quick and 'sufficiently developed; his skin was moist.

'He was ordered barley-water with mucilage, and a 'glyster of marsh-mallow decoction, which procured one 'stool before the next day.

'On the seventeenth, he was ordered six grains of 'ipecacuanha. He vomited twice, and brought up a 'great quantity of bile. He had no stool. He slept well 'in the night, and awoke in a state of mild perspiration. 'The next morning (the eighteenth) the yellow coating 'of the tongue had disappeared. It had a fine vermilion 'colour. The bad taste in the mouth was gone; the 'pulse was moderately frequent; the temperature of the 'skin almost natural.'

Here surely was encouragement to proceed with proper evacuants, supporting the strength of the patient at the same time by mild nutritive drinks and small quantities of wine; but it is not mentioned that any remedies were ordered for four days afterwards!

'From the nineteenth to the twenty-third, there was a 'slight febrile movement, anorexia, and the same state of 'tongue, and one liquid stool after a glyster. *Softening 'ptisans and two bouillons every twenty-four hours.*

'On the twenty-third, the tongue was red, the quick- 'ness of the pulse was considerably increased, the skin 'was of a burning heat, the abdomen painful on pressure. 'During the last twenty-four hours there were two liquid 'stools.

'This return (*récrudescence*) of symptoms was combated

'by the application of eight leeches to the arms, by 'barley-water and diet.

'During the day the abdominal pains became fearfully 'intense, and the patient began to vomit a great quantity 'of bile of a leek colour.

'In the morning of the twenty-fourth, we found the 'patient lying on his right side, his eyes sunk, his face 'pale, and cadaverous. The lightest pressure on the 'abdomen and the slightest movement of the body pro-'voke the most acute pain. The patient is tormented 'with continued nausea, which is followed from time to 'time, by the expulsion of mouthfuls of bile. The respir-'ation is loud and quick, and is performed solely by the 'elevation of the ribs; the pulse is very frequent, *misérable*, 'the skin without heat.

'The existence of peritonitis was looked upon as 'certain. M. Lerminier conjectured the cause to be a 'perforation of the intestines. *Forty leeches to be applied 'to the abdomen, oily fomentations, mitigated sinapisms to 'the legs in the evening, and linseed tea.*

'Twenty-fifth, at eight A.M.—The abdomen was less 'sensible, but more distended. When struck, it yielded 'a dead sound. Fluctuation was not to be discovered. 'The extremities were cold; the pulse filiform, and yet 'the eyes had a kind of natural expression; the intellects 'clear, and speech free. *Blisters to the thighs.* Death at 'five P. M.

'On opening the body of this patient, the small intes-'tines were found to be slightly united at some of their 'curves by means of albuminous flakes, (des flocons 'albumineux). The mucous membrane of the stomach, 'and of three-fourths of the small intestines, were every-'where sound, and of a pale colour; but for the space 'or rather length of a foot immediately above the ilio-'cœcal valve, there existed five or six oval-shaped

'elevations, which were white, as well as the mucous 'membrane that surrounded them. One of these was 'ulcerated in the centre; the bottom of which ulcer per-'forated the serous coat of the intestines; the mucous 'membrane which surrounded the larger elevated spots, 'was studded with a number of small white miliary 'pustules, which were scarcely elevated above its sur-'face. The large intestines and other viscera were in a 'sound state.'

This case is both instructive and remarkable, not only for the treatment, but also for the observations which M. Andral has made on it; some of which I must translate before commenting upon them.

'The principal circumstance,' he says, 'which makes 'this case worthy of interest, is the existence of a well-'characterised *dothen-enteritis* in an individual, who, 'until the period of attack, presented only the symptoms 'of a benign continued fever.' According to the symptoms it was a *bilious fever of little intensity.*

'The abdomen was soft and indolent in every part; 'the tongue without redness, presented a thick coating, 'which disappeared after the administration of an emetic. 'When it had operated, the gastric symptoms ceased; 'but there remained a little fever, and soon after a 'slight diarrhœa succeeded to the constipation, which 'existed from the beginning of the disease. This intes-'tinal flux did not occur until the eleventh day. Did its 'invasion mark the moment when one of the *plaques* '*exanthematiques* began to suffer ulceration? Almost at 'the same moment peritonitis occurred; the cause of 'which was rendered evident by the dissection: — the 'internal ulceration having, by its extent and depth, per-'forated the ileum.'

Observations on the preceding Case, and on the Common Bilious Fever in general.

The remarks which I have already made on the analogy between the inflamed, pustular, and ulcerated state of the intestines in the epidemic thrush fever, and the appearances of the same viscera in the dothinteritis of the French school, render it unnecessary for me to repeat the arguments by which I endeavoured to prove the great probability that these lesions were all of an atonic kind, and that they arose in the course of the fever from vitiated secretions acting on parts in which the smaller arteries and capillary vessels were already in a congestive state. One or two of the cases of typhus which terminated fatally, narrated by M. Andral, tend to prove that the pustulation arose from the topical irritation of worms. In fact, it appears to me, that vitiated secretions may in the course of typhus occasion the intestinal lesion called dothinteritis, but I deem it an error in practice to be turned aside from the old and approved treatment by the knowledge of an anatomical fact, seeing how easily it may be misinterpreted. In the cases already cited, and in many others mentioned in the third volume of the Médecine Clinique, it is obvious that the treatment by blood-letting failed; and it is really melancholy to reflect that the well-marked character of the disease, and the hints which nature gave in these cases, as well as the authority of old practical writers, should have been so little attended to.

A true bilious synochus, such as seems to have raged for some years consecutively in London and many parts of England, about two hundred years ago, has seldom occurred of late, except in solitary cases. It is quite a distinct disease from bilious typhus. The fever which is most analogous to it, is the bilious remittent of hot

climates, and more particularly the bilious remittent which prevails in various parts of the Mediterranean coast, in the south of France, and in Italy.

Since the time of Sydenham it has seldom raged as an epidemic in London or any part of Great Britain. This is probably owing to the cold, or at all events cool summers, which we have experienced for so many years past; for the true bilious fever, when it rages as an epidemic in Europe, is the effect of hot and humid weather, followed by sudden alternations of heat and cold in autumn. It is the offspring of climate and seasons, and is never contagious. In this it is essentially different from typhus with bilious symptoms, which may occur in individual constitutions, wherever the contagious typhus prevails. The brain is not the organ which is first affected in the bilious synochus. If it now and then assumes a typhoid character, it is only in consequence of improper treatment or of long duration.

The true bilious fever of Europe moreover differs most essentially from typhus in its precursory symptoms, as well as in its general character and method of cure. By far the greater number of patients suffer from a kind of congestive state of the liver for several days, nay, sometimes for weeks, before the real febrile paroxysm commences. They are troubled with languor, pains in the back and loins, loss of appetite, headache attended with giddiness, anorexia, and a bad taste in the mouth. But the greatest distinction between this fever and the true typhus with bilious symptoms, consists in this, that in the first, the patients not only bear, but indeed demand, emetics and purgatives during many days consecutively, in the first stage; a mode of treatment which would be often detrimental in a typhoid case.

A true bilious synochus may arise in solitary cases as well as from general causes. The first of the two pre-

ceding cases is a perfect type of one of the former kind. The exciting causes, the premonitory symptoms, as well as those which accompanied it on its regular accession, are truly characteristic. To me it appears that the case was misunderstood from the commencement, chiefly owing to preconceived opinions, and yet nature gave the strongest hints of what ought to have been done.

In this fever, the overloaded biliary ducts and the gall-bladder ought to have been relieved, and blood-letting to have been avoided. This last-mentioned remedy is a truly hazardous one in all bilious affections, without exception, in cold climates; in typhus with bilious symptoms, it is pernicious. Violent headache, redness of the face, burning heat of the skin, and even acute delirium, do not justify general and copious blood-letting. In any case where the colour of the skin, especially about the eyes, mouth, and temples, is very yellow, the urine much tinged with bile, the tongue exhibiting a thick yellow coating, and when there is a spontaneous or imperfect vomiting of bile, and complete anorexia; violent pain at the pit of the stomach, does not justify the use of the lancet; for such pain is of the nature of cramp, and arises from the acrimony and quantity of bile which has found its way into the stomach and duodenum.

A combination of an emetic with a mild cooling purgative medicament is the best remedy in the beginning; for instance, from half an ounce to an ounce of any purgative neutral salt, with half a grain to a whole one of tartrate of antimony. This generally excites both gentle vomiting and purging, and relieves the headache and abdominal pains. Its repetition must depend on the recurrence of the symptoms, and the effects produced by the first dose on the strength of the patient; and indeed the whole treatment of the disorder becomes one of mild but effective aperients.

In the year 1784, after finishing my studies at Edinburgh, London, and Paris, I visited Vienna, and during seven months witnessed the practice of Stöll and the other successors of the old Boërhaavian school, comparing it with what I had seen at home. The works of Van Swieten, De Haen, and Stöll, I presume, are sufficiently well-known to every informed physician, and therefore I deem it unnecessary to mention their opinions.

The bilious fever happened to be prevalent in the autumn of the year I have mentioned. It was attended with a symptom I had never seen in Great Britain, and which is rarely mentioned by British physicians as one which is common—I mean an acute parotis. It soon formed a large and painful abscess, except in very debilitated individuals. Its suppuration was looked on as a favourable crisis; and accordingly, this was promoted by poultices and fomentations. But the general treatment of the fever did not differ in principle from that which I have already explained, though some of the remedies were different in form.

Stöll, in his later years, had a prejudice against emetics; and I think neglected them too much. The inflammation and suppuration of the parotid gland appeared to me, judging from the practice of the other physicians, to be more frequently the effect of a want of the early evacuation of the bile, than to be a natural consequence of the fever, or an effort of nature. The evacuating remedy commonly resorted to in the General Hospital of Vienna, was the well-known Vienna draught or drink, with emetic tartar, and afterwards with sal ammoniac (muriate of ammonia) in the intervals,* small

* *Infusum Laxativum Vienneuse.*—℞. Fol. sennæ elect. ʒiii; passularum minorum ʒijs; rad. polypodii querni ʒiii; sem. coriand. ʒij; cremor tartari ʒss; aquæ ferventis lb. iijss; digerantur per aliquot horas et liquori expresso adde, mannæ opt. ʒviii; *dosis* ʒii. ad ʒiii.

doses of emetic tartar were occasionally given by some, but not by the generality of physicians. The decoctum rad. graminis, and appropriate doses of soluble tartar, were given freely after the first stage. Diaphoretics were not employed in this fever; which I deemed an improvement on English practice. The patient's strength was supported by nutritious drinks, and towards the later period of the disease, tonics and serpentaria were employed by some. In no case whatever do I recollect that calomel or any mercurial medicine was administered, but in lieu of it, the muriate of ammonia, with the extracts of chelidonium and taraxacum, together with the watery extract of rhubarb and soluble tartar, were given freely. On the whole, the practice was successful; nevertheless, I prefer the English practice, as far as the judicious employment of calomel in this fever is concerned. It is a sheet anchor, after the full evacuation of the primæ viæ has been obtained.

The necessity of administering this excellent and powerful medicine in the earliest stages of the bilious remittent of hot climates, and in doses which appear extraordinarily large to European physicians, is acknowledged, and indeed insisted on, by all the best English writers on that disorder. But such practice is by no means necessary, nor is it even safe in the bilious fever of temperate climes, if employed with the same freedom. Mercurial action carried to the point of salivation is always a hazardous state of body for any individual in a cold and variable climate; and as the functions of the liver and intestines are never so much paralysed in the common bilious synochus of Europe, as in the bilious remittent or yellow fever of tropical climates, this extreme practice is not called for.

But after the patient has been freed from the first great

collection of bile, by one, two, or three emetics and saline purges, and it is deemed imprudent either to repeat them, or totally to abstain from evacuants, I know of no safer nor more effectual a plan of treatment than to give calomel in small doses, either in combination with antimony, or with a mild eccoprotic, such as the compound rhubarb pill, and a solution of some neutral purgative salt in the infus. sennæ occasionally. The first of these combinations I venture to recommend especially as the best means of preventing, and also of curing, M. Bretonneau's dothenteritis, or pustulated state of the intestines: as the following case will best illustrate.

Some years ago, after my return to this country from Russia, I was requested to see a young gentleman, an artist; who, I was told, had been labouring for forty days under an inflammatory bilious fever!

When I first visited him, the albuginia of the eyes, and the skin of the face, neck, breast, and arms, were certainly of a yellow colour; his pulse beat 120 per minute, and was wonderfully strong and hard, considering the duration of the disease, and total loss of appetite. His disgust against food of all kinds was great, but he drank freely thin barley-water or plain water. The tongue was dry, and of a dark-brown colour in the middle, the edges and point moist, with a yellowish white slime or mucus. The abdomen was slightly distended, and he shrank when it was forcibly pressed, but moderate pressure did not cause him to complain. There was no pain in the region of the liver. He breathed quickly and freely, and could take a full inspiration. His intellects were free. He had no headache, and had not been delirious. He slept badly, and only for short periods. He complained much of general pain, uneasiness, and great weakness. The urine was of a higher

colour than usual, was scanty, and did not deposit any sediment, even after being allowed to stand for some hours. He had several scanty liquid dark-coloured stools daily. Sixteen ounces of blood had been taken from his arm in the beginning of the disease, on account of a violent headache, and the hardness and throbbing of the pulse: this relieved the headache, but had no influence on the quickness of the pulse, or on the other febrile symptoms. His bowels had been repeatedly evacuated in the commencement, by means of mild purgative remedies and enemata, and the medicine he was then taking consisted of small doses of the sulphate of magnesia, in the common saline diaphoretic draught with vin. antimonii. His pulse might have misled many; and to have judged solely by it, and by the pain on forcibly pressing the abdomen, he ought to have been immediately blooded; but it was clear from the long duration of the complaint, that active inflammation was not to be suspected; and I knew from long and repeated experience that chronic and passive inflammation of the intestines, or of the mesentery and peritoneum, each of which might be suspected, was not to be cured by loss of blood. If the liver was not in a congestive state, and I do not presume to say it was, there existed at all events a disordered secretion of bile; and therefore, acting in conformity with such symptoms and such conjectures, I ordered him to take a powder composed of half a grain of calomel and one grain of the pulv. antimonialis thrice a day, together with a small draught, containing five drops of laudanum and six of the tincture of digitalis. The laudanum was given in the hope of immediately arresting the frequent and inefficient stools, until the calomel and antimony acted. The digitalis was added on account of the frequency of the pulse. Sago, tapioca, and arrow-root, were to be his chief nutriment.

After the lapse of the first twenty-four hours, there was an evident improvement in the condition of the patient. The tongue began to assume a better and cleaner appearance. In the night he had been affected with an accumulation of phlegm in the fauces, which created a protracted fit of coughing, but it ceased towards morning, and did not return until the following morning. He had not had any stool.

I ordered the same medicines to be continued, but in order to relieve the bowels, I prescribed, in addition, a powder composed of ten grains of rhubarb and one of ipecacuanha; and, as a test of the suspected inflammation, I allowed the patient a tablespoonful of wine to be given every three or four hours, provided it did not produce an increase of pain, heat, or agitation.

The attending apothecary did not give the patient more than half of the rhubarb powder, being apprehensive of bringing back the diarrhœa. As he was still without any evacuation by stool, I insisted on the whole powder being given early the next morning. It was given accordingly; but the dose co-operating with the calomel, antimony, and rhubarb he had previously taken, produced several large evacuations which brought away much offensive matter, but also occasioned a sensation of great exhaustion and languor. The pulse, however, was softer, the tongue cleaner, and the skin a little moist. A few drops of laudanum were now given to him; and I ordered him chicken broth, thickened with tapioca, and allowed him half a moderate-sized wineglassful of claret every three or four hours, and the powders of calomel and antimony to be continued. On the fiftieth day of the disease he had a mild diaphoresis for several hours; his tongue was almost clean, and his appetite for food began to return, and from that period he became regularly convalescent.

The true typhus with bilious symptoms is a very distinct disease from the true bilious synochus. In the former, the brain and intellects are affected in the earliest stages, and the malignity of the disease in each case may be predicted with some degree of certainty from the more or less severity of the cerebral affection. It is often met with sporadically, owing to local causes. At the end of very hot summers and moist autumns it is more common. The bilious symptoms announce themselves in general at the beginning of the disorder by intense headache, vertigo, a great dislike for food, a bad taste in the mouth, a thick, yellowish-coloured, mucous coating on the tongue, yellow colour about the eyes, temples, and mouth, high-coloured urine, an offensive breath, and a burning hot skin. In bad seasons and cases, petechiæ appear, but a miliary eruption is more common.

In this fever the chief remedies are, first, emetics, very mild purges; afterwards, mineral acids, wine, and diluted alcoholic liquors and cinchona; great attention being always had to the function of the liver and the state of the bowels.

General doctrine of typhoid fevers; being an attempt to explain the phenomena, and to establish safe rules of treatment.

Experience teaches us that typhous fever frequently appears in solitary cases, as well as in the form of an epidemic. The causes to which single cases are most commonly traced are various; but all of them are debilitating agents,—such as damp habitations and want of food, a cold moist atmosphere operating for too long a time on an individual previously exhausted by bodily fatigue and want of nourishment; but as to quick or

sudden transition of temperature, either from heat to cold or from cold to heat, though numbered among the causes of typhus, they rarely excite this form of disease in any individual, except he be in a debilitated state of body at the time. Solitary, or (as they are called by medical writers) sporadic cases of typhus, though generally milder than those which arise from contagion, do not differ from the last in their nature, or in any essential character. In the preparatory stage, in the fully-developed paroxysm, and in the whole series of subsequent symptoms, whether the fever terminates favourably or fatally, and even in the appearances after death, the diseases are perfectly alike. Nay, what is of greater importance in proving their identity, is a fact of frequent occurrence, namely, that any single case of typhus, however accidentally produced, may, under particular circumstances, such as want of cleanliness, become a source of contagion to the neighbourhood, or to a village, or to a whole district. A case of this kind is mentioned by Dr. (now Sir Henry) Marsh in his essay on the *origin of fever*. (See Dublin Hospital Reports, vol. iv.*)

* Although the following observations may seem to some to be out of place, I cannot consider it as such, writing, as I pretend to do, for the advantage of young professional men.

In no country of Europe is contagious typhus in all its worst features so frequent (during peace) as in our sister island Ireland. The utter destitution of too many of the peasantry and labouring poor; their cold mud cabins; their more than half nakedness; together with the mental, as well as bodily suffering, which accompany want, are constantly in operation among the lower orders of that unhappy country, so that typhous fever is one of its most prevalent epidemics. Even in its capital, where there are so many noble institutions and charities for the relief of the poor, typhus takes its rise in its narrow lanes, not only in autumn, winter, and spring, but in almost every month of the year; and, consequently, in no city in Europe is there so much and such frequent opportunity of studying this disease, in all its worst varieties, as in the Dublin Hospitals. But this is not the only hint I have to

If it be true in philosophy, (as indeed all facts when accurately examined tend to prove,) that a similarity of action and effects, implies a similarity of causes; it is reasonable to conclude that the exciting causes of typhous fever, however different they are in their physical qualities, yet act by virtue of a principle which is common to all of them. From such a conclusion it is impossible to escape.

Poverty and fatigue; cold and damp habitations; exposure to cold winds, sleet, or rain, after an individual has been much weakened by severe or too long-continued exercise, must be supposed to act on the same principle with contagion if they produce diseases that are identical; and yet few things appear to be more dissimilar in their physical character than these causes.

If the principle of their action cannot be detected in the qualities of the causes, we may hope to discover it in the influence which they exercise on vitality, (considering this as a distinct force,) for, like every other force in nature, it may be opposed by others, and, consequently, its action may be greatly modified, or altogether neutralised and annulled, by such opposition.

The disturbing forces are of two classes, viz. mechanical and chemical; for no one surely will deny that organized bodies are subject to their influence as well as to that of vitality.

offer to medical students concerning this disease. To praise the Dublin physicians generally, either for their erudition or their medical science, or their practical talents, I deem unnecessary. He who is unacquainted with their works and merits, must either be ignorant of very important parts of medical literature, or else he must labour under illiberal prejudices. In the London Hospitals typhus is by no means a rare malady, but it seldom puts on that highly-dangerous character which it frequently assumes among the lower classes of the Irish, and, consequently, the London Hospitals do not offer so large a field for studying the disease, nor for the display of practical talent, as those of Dublin.

The nature of all these forces is equally obscure and undefinable. We have no means of attaining to a knowledge of force (considered abstractedly) except by its effects. But the phenomena of vital force are so distinct from those of the two physical forces just mentioned, that we can discover the effects of each of them in most cases.

It is the property of that mysterious force which is called *vital*, to combine a greater number of inorganic elements together, than can be effected either by mechanical or chemical attraction ; and we know it to be a fact, (though we cannot explain the reason of it,) that when organic molecules or germs are once formed, they exhibit movements which cannot be explained by the laws of any physical force ; and we further know, as a matter of daily observation, that life and health depend on the unobstructed and unopposed influence of vital force, and that all deviations from health imply the ascendency of chemical and mechanical forces over that of vital affinity.

The very first symptoms which announce typhous fever are those which characterise a remarkable diminution of vital force, especially of that which belongs to the brain. The intellectual faculties, the nervous sensibility, and muscular strength, are all impaired ; and, as far as we can judge, they are so at the same time on the first outbreak of the disease; proving to us in the clearest manner the influence of the disturbing causes.

Before proceeding further I take this opportunity to offer a practical remark which belongs to the first general effect of the exciting causes, viz. that the *curability* of any given case is in a great degree determined by the amount of their primary action on the brain. No doubt life may be shortened, even in mild cases of fever, by injudicious treatment, and unavoidable accidents; but in

some unfortunate cases, human art cannot interpose a remedy which is sufficient to overcome the death-blow that is now and then given by a very powerfully-exciting cause of fever, such as contagion, or even excessive fatigue, cold, and moisture, for this plain reason, that we do not possess the means of quickly restoring cerebral energy by any direct action on the organ, and all our indirect means are hampered and rendered useless by the disordered state of the functions through which we must operate.

This holds good equally in regard to malignant remittents, and even some intermittents, as to continued fevers. I mean such intermittent and remittent fevers as arise in some of the southern parts of Europe, and in hot climates, from highly-noxious miasmata in a concentrated state.

In this country, and in all the temperate climates of Europe with which I am acquainted personally, it seldom happens, that the brain is so much paralysed, (if I may use the expression,) even by contagion, and still less by atmospherical causes, as to take away all hope of recovery. Nevertheless, I am persuaded that there are few practical physicians of extensive experience, who, in the course of a long life, have not met with instances of the kind. I allude to cases of attaxique or typhoid fevers that commence with such decided enervation of the brain as proves the destructive influence of the exciting cause. The comatose symptoms which characterise this affection, are different in their nature from those which frequently occur, in the course of the fever, from sympathy with the stomach, from congestion of blood, or from serous effusion. All these last-mentioned cases are curable, if the strength of the patient be not too much exhausted, but I look on the first kind as fatal.

In this case the heart is so much enfeebled as not to be

capable of expelling probably one-third of its contents, and, consequently, the pulse is remarkably small, feeble, and frequent. A complete reaction, as it is called, never takes place; the extremities remain cold, and are always more or less of a purple hue. The only parts of the body which acquire a preternatural degree of heat, are the pit of the stomach and abdomen, where it may be supposed a congestion of blood takes place.

The most approved remedies, such as warm stimulating fomentations, blisters, and sinapisms, to the extremities, blisters to the head and spine, ammonia, mercury, and emetics, the abstraction of blood by leeches and cupping, and also from a vein, (though certainly not judicious in such cases,) have all been tried without effect. There are no signs of congestion of blood in the head in such cases. The brain may not improperly be said to be in a state of paralysis. Its peculiar function, by which it forms a stimulant for every other living organ, is, in the case I am alluding to, irrecoverably impaired; but according to the remaining strength of the patient he may continue to struggle several days before death occurs. When this happens, he sinks slowly and gradually into the grave without general convulsions, but now and then with subsultus tendinum and laborious respiration. He seems to pass from sleep to death. In these unfortunate cases wine and other stimulants seem to be indicated; but, as I have already stated, they make little or no impression on the system either for good or for evil. Such extreme cases are happily rare. With one solitary exception, the very few cases of the kind which I have met with arose from concentrated contagion. The solitary exception was the effect of the combined influence of great bodily fatigue and much anxiety of mind during a journey in wet and cold weather. The patient was a young man of an excellent constitution, and in the prime of life. When

I first saw him, which was on the second day of the fever, he appeared to be in a state of profound sleep, with a quick but not oppressed respiration. His pulse could scarcely be felt at the wrist, it was so small, and it could with difficulty be counted, on account of its quickness as well as its feebleness. He seemed to understand, faintly, questions put to him in a loud voice, and by the motion of his lips he appeared as if he tried to answer, but no sound was uttered. He died on the fourth day of the fever in the manner described.

To attempt an explanation of the nature of nervous energy, or to conjecture how it is formed, accumulated, or renewed by living medullary matter, is perfectly idle in the present state of our information; but so much we know, by observation and experience, that it is a motor force to other living organs of the body, and that the functions of such organs depend on its regular supply; that they are healthy and active when it is abundantly and freely supplied to them; that they are weak when it is scantily distributed; that they are interrupted when it is interrupted; and, finally, that they terminate when it ceases to be formed or distributed.

As soon as the brain is enervated to a certain degree by the action of contagion, or of any of the common causes of typhous fever, the heart, stomach, liver, and all the organs of secretion and excretion are also disordered, though, at first, in a slight degree in general, when compared with what takes place in the course of the disorder.

These affections of the heart and other viscera, do not follow each other like a series of causes and consequences, as is the case in the subsequent stages of fever; they are at first simultaneous phenomena, showing their dependence on one common cause.

The number of these first symptoms, and the succes-

sion of others which rapidly follow them, must now be studied in a careful manner, if the doctrine of the disease is to be improved; for, in fact, any doctrine which pretends to be complete, must embrace the explanation of every symptom. A slight degree of reflection ought to convince us that this cannot be accomplished by gratuitous suppositions, far less by the invention of imaginary causes; but by first collecting all the facts, and then examining them by the light which an improved and gradually-improving physiology throws on them. By such means alone can the pathology of fever, or indeed of any disease, be advanced. All conjectures, therefore, concerning general proximate causes must be abandoned.

On the affections of the heart and circulation of the blood in adynamic fever.

Of the numerous effects which flow simultaneously in consequence of the suddenly-diminished energy of the brain, the quick action of the heart, and the pretended quickness of the circulation of the blood, are the first which ought to engage our attention, not only because they are the most remarkable of all the phenomena, but because they are the source of very many alarming tertiary symptoms. An individual, at the moment the energy of the brain becomes impaired by the action of the causes of fever, may be considered as possessing nearly his usual quantity of blood. In this condition the heart is suddenly weakened, and it follows as a necessary consequence, that the pulse must be quickened, and also be small and weak in proportion to its quickness. (See First Commentary.) This symptom accompanies the disorder from its first attack to its termination; for owing to the sudden diminution of muscular power in the ventricles, those cavities either do not contract themselves

completely, so as to expel the whole of their contents, or else from their increased irritability they contract themselves before they are fully distended.

Let us now turn our attention to the arterial system, which, like the heart, has suffered a loss of vital tonicity.

It has been proved, as was formerly mentioned, by numerous experiments, that owing to the perfect elasticity of those vessels, they accommodate themselves to their contents, however small; and, consequently, when the heart, from being much weakened, throws out little blood at a time, the capillaries may contract so as to admit only one globule of blood, and even to expel that by further contraction, if not supplied with fresh portions of blood. But if the heart, instead of expelling an ounce and a half of blood at each contraction, as in health, does not project more than three-fourths of an ounce, or even less, (and the quantity propelled may be easily calculated by the number of pulsations per minute, the whole quantity of blood being supposed to be nearly thirty or thirty-six pounds in weight,) it is clear that the smaller arteries and capillaries will shrink in their diameter; and hence, the paleness, and shrinking of the features, and of the whole surface of the body, during the first stage; and hence, also, the diminished heat, the frequent sense of chilliness and suppression, or rather diminution, of the secretions.

Whatever quantity of blood the aorta and pulmonary artery receive from their respective ventricles, it is evident that for the purposes of health, that quantity must be propelled with facility and in due time through the capillaries; but we are furnished with experimental evidence to prove, that the greatest disturbances of capillary circulation arise when the heart is much debilitated. Both Spallanzani (*Expériençes sur la Circulation*) and Waydemeyer (*Untersuchungen über das Kreislauf*)

have proved, that as soon as an animal is much weakened, the motion of the blood in the capillary vessels becomes undulatory, remittent, and even intermittent, during the diastole of the heart; and it is surely easily understood why this disturbance should take place in a debilitated animal during the diastole of the ventricles, since all pressure on the blood is withdrawn at these moments, except such as arises from the contractile tonicity of the arteries, which, however, partake of the general weakness. This is the real state of the capillary circulation in the last stages of typhus when the extremities become cold and livid. Let us examine a little farther the condition of the larger arteries. They, like the heart, have lost a portion of their vital elasticity, and therefore they press with less force on their contents, so that, receiving fresh additions of blood from the heart, and meeting with opposition from the capillaries, they become gradually loaded with blood; offering a state of arterial congestion. In proportion to this plethoric state of the larger arteries, a sense of oppression, pain, and heat, is felt all over the body, but especially at the præcordia, together with throbbing in the temples, headache, and now and then with vertigo, delirium, and all the other phenomena of the hot stage of the paroxysm.

The pulse is small and contracted, as well as quick, in the first onset or cold stage of fever, because of the sudden diminution of cerebral energy, and defective contraction of the ventricles; it becomes fuller in the hot stage, in consequence of the blood that is thrown upon the heart and larger arteries, by the causes just assigned.

It would be a great error in observation, as well as in reasoning, if it were imagined that the disordered functions of the body really arose in succession at the commencement of fever. This is not the case, for every vital

organ is more or less affected at one and the same time, which gives rise to a suspicion that the remote causes, and more especially contagion, may act on the fluids generally before the solids are affected, especially when we reflect that there is always a lapse of time between the application of the remote causes and the breaking out of the disease, during which period there is very little or no apparent disorder in the system. But this curious question I shall examine more minutely immediately. As soon as the brain and heart are affected a great variety of morbid changes occur in every part of the body, and whoever undertakes to write a complete history of fever, must endeavour to make a complete analysis of these affections, as far as the progress of the auxiliary sciences enables him to do so. At my very advanced period of life, I cannot pretend to undertake, far less to accomplish, such a great task; all I can do is to offer a few hints to others on the leading questions.

Does contagion act primarily on the fluids or on the solids of the human body in typhous fever? Is the blood contaminated, and in what manner, in the course of the disease?

There are many reasons for thinking that previously to the affection of the brain, and consequently before the rigor and true paroxysm of the fever, the blood and circulating fluids of the human body suffer metamorphoses, and are chemically vitiated by the operation of the remote causes, but more especially by contagion. That contagion, however sudden its influence on the brain may appear to be in many cases, first acts on the blood, seems to me a very probable hypothesis; but the examination of this curious and interesting pathological question requires a patient discussion.

Of all the parts of our organisation, on which the phenomena of life chiefly depend, the blood appears to offer the least resistance to the agency of chemical forces. As soon as it escapes from a wound it loses its fluidity, and then it undergoes decomposition, in consequence of the affinities of some of its constituent elements for those of the atmosphere. In this case, both the blood and the atmosphere give and receive from each other some of their respective elements; and new compounds are formed, which are chiefly of a gaseous or volatile nature: I must presume my readers to be perfectly acquainted with the number and nature of these facts. But of late years the attention of chemists has been turned to other curious chemical phenomena, though they also, like the former, are dependent on the laws of chemical attraction.

The cases alluded to are of two kinds; by way of distinction they have been called chemical metamorphoses and transformations. In one case, the disturbing agent acts solely by virtue of its contact with the compound body which it has the power of decomposing. It yields none of its constituent elements to that body, and it receives none from it. If it be platinum or silver which produces the disturbance, no change whatever is observed in the metals after they have performed their function; if the disturbing agent be sulphuric acid, it is found unchanged in quantity and quality after it has transformed a chemical compound into a new substance, as in the instance of the conversion of starch into gum, and then into sugar by means of that acid.

But there are other facts belonging to the subject of chemical metamorphosis of an equally remarkable character, which Dr. J. Liebig, in a work of great ingenuity and talent, applies as the basis of a theory for explaining

the action of contagion, and other animal poisons, on the living body. See his *Chemistry in its Application to Agriculture and Physiology.*

To pass over in silence so important a doctrine as this in a commentary like the present is impossible, though it may not appear that it can benefit practice much in its present imperfect state; but as it embodies under general principles many very obscure and hitherto unexplained facts, its influence on pathological reasoning will be felt and acknowledged by every competent judge.

The generalisation of all the facts on which Liebig's theory of the action of contagion and other animal poisons is founded, may, in my opinion, be thus expressed: *If the exciting agent of chemical metamorphosis be a compound body, which is in the act of decomposition, it will reproduce itself* AD INFINITUM, *provided the compound body on which it acts contains elements fitted for such an end.* Thus, if yeast be mixed with a solution of sugar, to which gluten has been previously added, not only will the saccharine solution be converted into alcohol and carbonic acid, but new yeast will also be formed; whereas, if no gluten, nor any substance containing it, be added to the solution of sugar, no new yeast will be formed, and the produce of the fermentation will only be alcohol and carbonic acid. That something very similar to this takes place in the living human body, from animal matter which is in a state of putrescency or of spontaneous decomposition, Dr. Liebig argues from the following facts, and in the following manner.

'It is a well-known fact,' (he says,) 'that when blood, 'cerebral substance, gall, pus, and other substances, in a 'state of putrefaction, are laid on fresh wounds, vomiting, 'debility, and at length death, are occasioned. It is also 'well known that bodies in anatomical rooms frequently

'pass into a state of decomposition, which is capable of 'imparting itself to the living body; the smallest cut 'with a knife which has been used in their dissection 'producing in these cases dangerous consequences.

'The poison of bad sausages belongs to this class of 'noxious substances. Several hundred cases are known 'in which death has occurred from the use of this kind of 'food. In Würtemburg, especially, these cases are very 'frequent, for there the sausages are prepared from very 'various materials. Blood, liver, bacon, brains, milk, 'meal, and bread, are mixed together with salt and 'spices; the mixture is then put into bladders or intes- 'tines, and after being boiled is smoked.

'When these sausages are well prepared they may be 'preserved for months, and furnish a nourishing savoury 'food; but when the spices and salt are deficient, and 'particularly when they are smoked too late or not suffi- 'ciently, they undergo a peculiar kind of putrefaction, 'which begins at the centre of the sausage. Without 'any appreciable escape of gas taking place, they become 'paler in colour, and more soft and greasy in those parts 'which have undergone putrefaction, and they are found 'to contain free lactic acid, or lactate of ammonia; 'products which are universally formed during the pu- 'trefaction of animal and vegetable substances when 'mixed.

'The cause of the poisonous nature of these sausages 'was ascribed, at first, to hydrocyanic acid, and afterwards 'to sebacic acid, although neither of these substances had 'been detected in them. Bnt sebacic acid is no more 'poisonous than benzoic acid, with which it has so many 'properties in common; and the symptoms produced are 'sufficient to show that hydrocyanic acid is not the 'poison.

'The death, which is the consequence of poisoning by

'putrefied sausages, succeeds very lingering and remark-
'able symptoms. There is a gradual wasting of muscular
'fibre, and of all the constituents of the body similarly
'composed; the patient becomes much emaciated, dries
'to a complete mummy, and finally dies. The carcase is
'stiff as if frozen, and is not subject to putrefaction.
'During the progress of the disease the saliva becomes
'viscous, and acquires an offensive smell.

After stating that numerous experiments had been made in vain, with a view to discover a specific poison in these sausages, he proceeds thus:

'Sausages, in the state which has been described, exer-
'cise an action upon the organism, in consequence of the
'stomach, and other parts with which they come in con-
'tact, not having the power to arrest their decomposition;
'and entering the blood, in some way or other, while
'still possessing their whole power, they impart their
'peculiar action to the constituents of that fluid.

'The poisonous properties of decayed sausages are not
'destroyed by the stomach, as those of the small-pox
'virus are. All the substances in the body capable of
'putrefaction are gradually decomposed during the
'course of the disease, and after death nothing remains,
'except fat, tendons, and bones, and a few other sub-
'stances which are incapable of putrefying in the condi-
'tions afforded by the body.'

He then argues, that every organic substance which undergoes a specific kind of putrescency, has the power of transforming the blood or some of its elements into a similar putrescent matter; adding, by way of confirmation of this opinion, that Colin 'has proved, that muscle,
'urine, cheese, cerebral substance, and other matters in a
'state of putrefaction, *communicate their own state of*
'*decomposition* to substances much less prone to change
'of composition than the blood.'

That specific poisons are generated in the human body in the course of many diseases, is too well-known a fact to be denied. The virus of small-pox, measles, scarlatina anginosa, plague, and syphilis, are so many instances of the kind. The theory of their formation is a question of great interest. It would appear that there are two ways only by which any rational explanation of the phenomena can be given. Either they are generated in the blood, according to the general law of chemical transformation, or they are specific secretions. I know no facts of any value which can be adduced in favour of the last of these opinions; but there are many which support the first, beside those which have been already mentioned.

Between the first moment at which any virus or animal poison (the effects of which can be discerned by sight) comes into actual contact with living blood, a certain lapse of time, which is different for each specific virus, takes place before the transformation of that portion into a similar virus is completed. When we introduce into the minutest wound we can make, for instance that made by the point of a needle a minute quantity of variolous matter, we observe in the first place such a slight inflammation as a wound of the size would naturally occasion, and if a comparative trial is made with an uninfected needle of the same size, experience teaches us that the inflammation and the wound of the clean needle are soon healed. But the inflammation produced by the infected needle goes on increasing, showing us, agreeably to the doctrine of inflammation, that a disturbing cause is present and continues to act. The small quantity of virus exercising its influence as an excitor of chemical transformation to the few particles of blood with which it comes into contact, immediately destroys their affinities, and causes them to assume a movement and arrangement similar

to that of the elements of the excitor itself; and in this way new virus is formed. This newly-formed virus is collected in the form of a pustule, surrounded by considerable phlegmonous inflammation, which seems to put a stop to the local contamination; but part of this newly-formed virus being absorbed, and being now mixed with the whole mass of blood, it commences its action as a chemical agent *de novo*, gradually transforming such constituent elements of our fluids as are susceptible of this kind of chemical transformation; and when that has increased to a certain degree, (and not before,) then the vitality of the brain and of every other organ of the body, is synchronously affected, and the febrile paroxysm commences.

When variolous virus is diffused in the atmosphere, and instead of being introduced into the body by a wound is imbibed through the pores of the membranes of the nose and lungs, still a delay takes place between that epoch and the commencement of the fever, during which the chemical transformation of some of the constituents of the blood is progressing.

These facts and their explanation are applicable to the action of all kinds of virus which produce a specific disease, whether it be that of small-pox, the vaccine disorder, measles, contagious typhus, the plague, scarlet fever, or syphilis, &c.

If we reflect on the natural consequences of the functions of nutrition and assimilation, we ought not to be at a loss to understand why the blood is at all times one of the most complex fluids which can be imagined, and, as such, very liable to chemical action. Only a part of it is vitalised, namely, the red particles and the coagulable portion. The serum contains many substances either in solution or mechanically mixed with it, on which the

vital force does not act, and which are therefore much disposed to chemical transformations and action.

Liebig's doctrine of the action of animal poisons consists in a supposed gradual transformation of some of the elements of the blood only into a compound of similar chemical qualities with the virus which produces the disturbance; and I confess I am much inclined to think that it is chiefly on the serum of the blood, which is so little if at all vitalised, in which the metamorphosis takes place.

Many chemical irritants are formed during the very first processes of digestion, from superfluous food which is not converted into perfect chyme and chyle: many others result from the effete or dead materials of different tissues, which are absorbed and conveyed into the blood after they have served their several purposes; and hence it happens that in infancy, childhood, and youth, during which great quantities of food are generally taken and the process of growth is rapid, the blood is particularly disposed to the influence of contagious disorders. We are indeed kindly provided with special organs for the elimination of these chemical irritants: but the blood can never be wholly free from them, and a multitude of causes may weaken the functions of excretion. If, therefore, any contagious virus (animal matters undergoing specific putrescency) finds its way into the blood, it is reasonable to think it cannot fail to find plenty of materials on which to exercise its affinities.

So long as any specific virus or contagion finds materials in the blood, which, by the law of chemical transformation, it can set in motion and gradually convert into a compound *ejusdem generis*, so long does the disease continue. The disorder ceases when such elements have all undergone the transformation or are exhausted.

In febrile diseases of a contagious kind, the duration of the process is limited, because the total loss of appetite, and the loss of fluids by excretion, cut off the supply of fresh materials; but in infectious diseases, in which food is plentifully taken, such as syphilis and yaws, the chemical transformation goes on progressively until every vital function is disordered, and the whole vitality at last overcome. The experiments made in the venereal disease by Dr. Fricke of Hamburg, in the hospital of that city, prove incontestibly that not only the primary but all the secondary symptoms of that disease, in their most advanced stages, can be made to disappear solely by the hunger method of cure; that is, by reducing the patient to a very low diet, and by purging him daily with Epsom salts. A great number of these cases I saw during a short stay I made about twelve years ago in Hamburg, and I can vouch for their truth. Chancre, buboes, ulcers which were rapidly destroying the tonsils, palate, and nose, syphilitic blotches and scabs, affections of the periostium, were all apparently (Dr. Fricke says permanently) cured, without the administration of a single particle of any mercurial medicine. The daily administration of a moderate dose of sulphate of magnesia, confinement to bed, abstinence, and great cleanliness, were the only remedies employed in many of the worst cases.* The West Indian doctors may take a hint from this, for the treatment of yaws; but I do not pretend to say that the starving plan will cure that disorder. I have no experience of the kind. All I wish to state is that the plan is worth trying.

Some individuals resist, as is well-known, the inoculation as well as the imbibition of any virus, whether it be that of plague, small-pox, measles, or syphilis. In such a case, we must suppose, either that the force of vitality

* See Extracts at the end of this volume.

in such individuals counteracts all chemical agency, or else, that their blood at the time has not those elements which are fitted for the chemical metamorphoses; and this last conjecture is strengthened by a knowledge of the fact, that after the lapse of some time the same individual no longer resists the action of the same virus.

I now proceed to consider what changes the blood may be supposed to undergo during the course of the fever.

Without attempting too minute details, in order to discover the specific alteration which the blood undergoes in the course of the fever, it is sufficient to consider what must be the effects of the want of nourishment on the one hand, and the retention and necessarily rapid accumulation of innumerable chemical stimulants on the other. The disordered functions of the digestive organs accounts for the first, and that of the capillary arteries and of the organs of secretion and excretion for the others.

The discoveries of modern physiology teach us that in a state of health, the blood in its passage through the minute arteries and capillaries, suffers a loss before it reaches the smallest veins. Part of the liquor sanguinis passes through the pores of the minute vessels, and supplies to each organ and each part through which it circulates, the elements of their nutrition and renovation. It is asserted by some that the blood receives from the parenchyma other matters in exchange, which is probable, but not fully proved. What appears to be certain is, that owing to the loss of some elementary parts, which are withdrawn from the blood in the processes of nutrition, other elements are no longer held together by vital or organic laws, and, consequently, they fall under the influence of chemical affinities. When the blood reaches the veins, it is found to contain urea and lithic acid, together with a great number of chemical salts, especially

phosphates, tartrates, and chlorides; which in a state of health are separated from it, by the kidneys, the skin, liver, and other emunctories.

When the blood is poured into the left ventricle of an individual who is in perfect health, in order to be distributed throughout the body, it has been freed from many of these superfluous acrimonies. But it is easy to imagine, as indeed it is reasonable to conclude, that in the commencement of typhoid fever, and more especially in its progress, the chemical compounds and effete elements are not thrown out of the body in sufficient quantity in any given time, owing to the weak action of the heart, and consequently, the blood does become really contaminated, and this contamination must increase as the disease proceeds. The conjecture, therefore, of the old humoral pathologists was not altogether wrong as to the existence of acrimonies in the blood: but they committed an error in supposing them to be the cause instead of the effects of the disorder.

The peculiar fœtor or odour of the exhalations from the skin and lungs of patients labouring under the advanced stages of typhus, and the chemical composition of their urine, all demonstrate the formation of chemical compounds which do not belong to health; and it is, therefore, reasonable to suppose that they are to be considered as effects of the disordered circulation, they become gradually increasing causes of great disturbance from their accumulation. In fact, the chemical process has begun, and gradually gains an ascendency as the vital energy of the brain diminishes.

It must be candidly admitted, that however ingenious and plausible Dr. J. Liebig's theory of contagion may be, it is not as yet of any practical value. It exhibits causes of great and general disorder, but it does not explain all the symptoms, far less does it indicate any new principle of

treatment, or any new and important remedy; yet I deemed it impossible to pass it over in silence, seeing how strictly (though abstractedly) scientific it is.

It matters not, in a practical point of view, whether the brain and other vital organs are primarily or secondarily affected; that is, whether contagion acts immediately on the nervous system, or mediately through some preliminary contamination of our fluids. The results are the same; and it is to these results that our attention and efforts must be directed.

Until chemistry discloses to us the number and nature of the chemical irritants which are formed in the blood during the course of a fever, and of the existence of which there can be no doubt; until, moreover, it enables us how to distinguish the specific action of each of these chemical irritants; such, for instance, as those of the lactates, phosphates, muriates, acetates, &c.; and further, what are the appropriate chemical agents that we can introduce with safety and effect into the system, with a view of correcting them, we must direct our treatment on the principles of a rational empyricism, which is derived from our knowledge of the affections of the living solids, more especially of those of the brain and nerves, of the heart, and vascular system, and of the secreting and excreting organs.

First Effects of the Causes on the Stomach and Digestive Organs.

So essential does the regular supply of the cerebral stimulant or nervous energy appear to be to the healthy functions of the different organs of the human body, that as soon as its usual formation or distribution are affected by any cause whatever, either moral or physical, the effects are immediately seen on the whole or in

several of the vital functions. In no one is this more sudden or remarkable than in the digestive organs.

No sooner is the brain affected by any of the remote causes of fever, than a loss of appetite, and a dislike of food, are perceived. There is also often a clamminess in the mouth, a want of taste, sometimes headache and nausea; showing how much and how immediately the stomach is affected, not as a secondary effect from disordered circulation of the blood and altered secretion of the gastic solvent, but as an almost instantaneous consequence of the cerebral affection.

From the experiments and observations of Dr. Beaumont on the stomach of Alexis St. Martin, experiments so interesting in themselves, and so rich in their application to practical pathology, he proves that febrile commotion, as well as sudden emotions of mind, occasion an almost immediate change in the vascular appearance and condition of the inner surface of the stomach, and also in the secretion and sensible qualities of the gastric liquor. Whatever disturbed the nervous system of Martin, also disturbed the villous coat of the stomach and its secretions. When St. Martin suffered from any febrile attack, with a quick pulse and dry tongue, the secretion of the gastric liquor was suspended, and any food which was swallowed in such a condition of stomach, remained undigested for upwards of twenty-four hours, and consequently naturally aggravated the general symptoms of disease.

It is surely not unreasonable to suppose that similar effects and derangements may occur in the whole of the intestinal canal, also in the liver, the pancreas, spleen, and, in one word, the whole chylopoetic and digestive organs; and for the very same reason, namely, their dependence on the influence of the brain. A more complicated disease, therefore, than typhus, even in its first stage, cannot well be imagined; and they who think of

simplifying its doctrines, by ascribing all its phenomena to one proximate cause, such as topical inflammation, appear to me not to have had due regard to the true character and origin of its symptoms.

The vitiated secretions of the stomach, intestines, of the liver, peritoneum, lungs, or of any membranous, part are real topical irritants, which, in concurrence with weakened nervous power and a very disturbed circulation, soon produce local inflammations, pustulation, and even ulceration. This is perfectly intelligible, and consequently, to trace all these numerous and complicated effects to the primary affection of the brain and its nervous energy, appears to be a fair deduction from a due consideration and estimation of the facts.

According to the influence of seasons, frequently to that of local climate, and fully as often to idiosyncracy, the disordered affection of one or more of the functions that are the most essential to health, may predominate over others in typhoid fevers; and hence its great varieties. Thus some epidemics of this fever may be characterised with coma and delirium in the very beginning, others with bilious symptoms, others with glary accumulations in the stomach and intestines, some with diarrhœa, and again others with costiveness; and as the disease proceeds, and the physical constitution of the blood becomes vitiated from the effects of deficient nutrition and retention of chemical acrimonies, the disposition to passive inflammation and to gangrene must also increase.

In a former part of the Commentary I have endeavoured to prove from analogous phenomena in analogous febrile diseases, as well as from the results of practice, that the inflammation of mucous and serous membranes, which are so frequent in all adynamic fevers, are of a truly asthenic character, and ought not to be treated by blood-

letting and debilitating agents, but by an opposite class of remedies.

At the risk of being accused of repetition, I again entreat the reader to consult the third volume of the Médecine Clinique of M. Andral, for many convincing proofs of the truth of this opinion, provided he deems the united testimony of practical physicians for two centuries past, and many of those of the present day, to be insufficient for such a purpose.

The whole of the first commentary, as well as some of the present one, must convince every one who reads them, that I am not an enemy to hypothetical reasoning in medicine, when it is conducted by the rules and restrictions which belong to inductive logic; but it is to the hasty generalisation, which men of ardent imagination, as well as those who have a less excusable motive, namely, the desire of quickly gaining temporary fame by mere novelty of opinions, against which I contend.

Although we may never acquire a more perfect knowledge of the living principle, or of cerebral and nervous force or energy, than we do at present, still as many of its affections are objects of observation and experiment, the accumulation of facts relating to them must tend to the detection of unsound hypotheses.

It is under this impression that I pursue my object in attempting to establish indications of cure which are founded on the explanation of the symptoms.

Keeping in remembrance the facts previously enumerated, that the chief action of the common causes of typhous fevers is on the brain and nervous system, and knowing by experience that we do not possess a remedy by which we can restore its lost energy, except through the medium of the heart's action and the functions of the digestive organs, we ought to direct our chief attention to them in the first instance.

General Principles for the Treatment of Typhoid Fevers.

To allay the great disturbance which takes place in the action of the heart and circulation of the blood in fever, has always been an object of anxious concern to practical physicians, however much they differed in their theoretical opinions. The means proposed for effecting this object have necessarily varied with their doctrines, when not guided solely by empirical imitation.

In the hope of discovering what is the safest manner of proceeding, let us first attend to the process of unassisted nature, when she accomplishes the cure of a paroxysm.

The analogy between the first stage of a continued idiopathic fever and the shorter one of an intermittent, is perfect in every respect, except in degree and length of duration; but after the first stage is passed, the resemblance ceases. Nevertheless, this justifies the conclusion that the different form of these fevers arises from a difference in the force or dose of the causes, rather than in a specific difference in their mode of operation, or of differences of individual idiosyncracy in the patients.

The process of nature, by which a paroxysm of ague terminates favourably, is by profuse perspiration; that is, by the loss of a large quantity of circulating fluids. But this fluid is not blood. It consists of a portion of the liquor sanguinis, which has lost all vitality, and is loaded with chemical compounds that have been formed during the cold and hot stages of the paroxysm; during which the excretions by the urine, by the skin, and by the lungs, have been suppressed.

The quantity of fluids thus ejected is very great. What we further observe is, that in proportion as the perspiration flows freely, the disturbance in the circula-

tion, the burning heat of the skin, the dryness of the mouth, the headache, and general pain all over the body, gradually subside, and the patient at last commonly falls into a refreshing sleep, during which, as after fatigue in health, the brain recovers its lost power and energy, with this great difference, however, that in health the renovation of the cerebral force during sleep is complete, but in the intermittent fever it is only partial. In like manner, the lost vitality of the heart and vascular system is gradually restored; and the plethora being thus cured, by a natural process, the balance between the quantity of circulating fluids and the forces which circulate them, the pulse returns to nearly its natural standard; and indeed every organ of the body partakes in a similar manner of the beneficial change which has taken place from the profuse perspiration and the return to natural sleep. The appetite begins to be awakened, and the secretions and excretions to be renewed.

I hope the following attempt to explain the return of the paroxysms, although it interrupts the inquiry for a short time, will be pardoned, as it is not my intention to write a commentary on agues afterwards.

It must appear surprising, I think, to every reflecting person, that although patients are removed from the influence of the malaria which produce intermittents, the paroxysms return, at their usual periods, with as much violence as at first; and, indeed, in some cases, sometimes, with still greater violence. The patient may have fled from the marshy district, in which the fever raged, to a very remote and dry one, and yet, the paroxysms return after twenty-four, or thirty-six, or forty-eight hours, as if he were repeatedly exposed to the primary cause.

To suppose that intermittent fevers arise from the imbibition of poisonous exhalations, which produce a chemical effect on the blood as animal poisons do, is not

rational; for in such a case, the intervals of febrile action which characterise intermittents could not be explained.

The cause of each succeeding paroxysm must, therefore, be sought for in some other part of the body of the patient, who sometimes labours for many months under the complaint.

At the termination of a paroxysm he feels himself to be comparatively well; but his health is by no means perfectly restored. The digestive organs remain weak; and their secretions, therefore, are probably neither sufficient in quantity or quality; the appetite is more or less impaired, and the concoction of the food imperfect. Patients afflicted with ague often suffer during the intermission slight headache, thirst, and always, more or less, general languor; and there is reason to believe, from the great sediment which is formedin the urine, that the functions of the whole chylopoetic viscera are disordered, and consequently, that vitiated secretions take place, not in the stomach only, but also in the intestinal follicles. When these secretions are accumulated to a certain amount, they act like any other generally debilitating power, and consequently, after a longer or shorter period, according to the rapidity of their formation, their nnmber, and quantities, and the idiosyncracy of the individual, they affect the brain and nerves *de novo*, and through them the whole system, just as the external poison or miasm did at first.

Marshy miasmata have always been considered as specific causes of intermittents; but as those diseases often arise solely from cold damp air, and, now and then, from a cold east wind alone, and also from great and sudden distress of mind alone, of which I have seen two cases, and more are mentioned in the annals of medicine, it is obvious, that the production of an ague does not depend on one specific poison, but on a variety of

debilitating causes, any one of which is sufficient for the effect; therefore there is no good reason for limiting the causes to external agents, especially when once the disposition to this malady has once been formed by a regular paroxysm. Stomachic and intestinal colluvies may generate all succeeding paroxsyms, the body being then disposed to such disorder. This opinion is strengthened when we reflect on the means of cure, which consist chiefly of tonic remedies, such as the quinine, a strong decoction of the bark of the salix tatifolia or S. alba, and other astringent bitters, or aromatic bitters with wine, dry air, and a due diet.

But although, in nearly eighty cases out of a hundred, agues in this country may be cured by quinine or willow bark alone, yet they fail entirely in some obstinate cases of the malignant Moldavian intermittents, of which I have had many cases among the officers of the Russian army on their return home. In such cases, however, the cure is conducted on the general principle of restoring the digestive organs to their proper functions, which is to be effected by small doses of calomel, or the pilula hydrargyri combined with other alterative remedies, as they are commonly called, which require to be varied according to individual cases. The taraxacum with senna, the blue pill combined with the extracts of chelidonium, taraxacum, and soap, or the same pill in small doses with the compound rhubarb pill, Ph. Edin., I found to be the best remedies, and sometimes to these alone, without a grain of quinine, very obstinate cases of agues have yielded.

In agues, spontaneous evacuations of other kinds, besides that of perspiration, sometimes occur during the paroxysm, but none of them appear to be critical or even salutary. During the cold stage, and especially towards its termination, vomiting now and then takes place, and

when it happens once or twice, it is apt to recur with every succeeding paroxysm until convalescence begins; but this evacuation brings little or no relief with it. A bilious diarrhœa occurs occasionally in some individuals and seasons. A moderate epistaxis relieves the head when it occurs; but a profuse one does harm, causing an anticipation of the succeeding paroxysms.

To return to the subject of continued fevers. Experience has taught the great bulk of medical men that every attempt to imitate nature by prematurely forcing copious perspiration in continued fevers, is in general vain; and that when we succeed in producing one, the event is very doubtful. There are, indeed, one or two cases recorded of the speedy and successful termination of typhus, by means of a large dose of James's antimonial powder, which operated both by vomiting and profuse perspiration. In my hands both this medicine, emetic tartar, combined with opium, and Dover's powder, (pulv. ipecacuan. comp.,) have failed, and it is now a great many years since I totally relinquished all desire of repeating the trials. In most of the cases these sodorifics greatly agitated and debilitated the patient, and in one unhappy instance, the patient being of a very feeble constitution, a large dose of the pulv. antimonial. Ph. Lond. produced such profuse perspiration as weakened the powers of the heart rapidly, and the perspiration passed into a colliquative sweat with cold extremities, all attempts to relieve which were fruitless. After this, I never tried large doses of antimonials in any period of the disease.

The process of unassisted nature in curing the long protracted paroxysm of a continued fever is conducted on the same general principle as that which she observes in the paroxysm of an ague; that is, she reduces the quantity of circulating fluids until she brings about an equilibrium between them and the weakened forces which

are to propel them, and when this happens, a critical sweat now and then occurs. But in agues the energy of the brain being less affected than in typhus, the heart and arteries exert greater influence, and consequently, the canals through which perspirable fluids pass, are sooner forced open by the quickened action of the heart and continued pressure of blood a tergo.

In adynamic fevers of the continued kind, such as typhus, the mass of circulating fluids is slowly reduced by abstinence, by the increased hatitus from the lungs and by insensible perspiration. Still the natural process of cure, both of ague and continued fever, is the same; and a question of great importance in practice arises, how far it is justifiable on the part of the physician to interfere and to take on himself the responsibility of the event.

According to the opinion I have advanced, it would appear to be perfectly reasonable to withdraw blood in the first stage of the disease, and to those who believe that local inflammations of an active kind are already formed or about to be formed, in so early a period of the disorder, the necessity of such practice must appear to be still more urgent. But let it be recollected that there is little analogy between the process of perspiration as an evacuant and the withdrawing of blood, as far as their effects on the energy of the brain and powers of the heart are concerned. By the former a quantity of fluids is thrown out, which are not only deprived of all vitality, and consequently, are hurtful if retained, on account of the numerous chemical compounds which soon contaminate the blood; but it is not by taking away blood itself that such contamination can be cured. Even an ounce of blood which is withdrawn, not only leaves the remainder charged with chemical compounds of a hurtful quality, but also deprives the blood of a

proportion of vitality on which the restoration to health depends. Hence the alarming collapse which so frequently follows the imprudent use of the lancet in typhoid fevers.

The results of my experience concerning blood-letting in typhoid fevers amounts to this :—that it is a hazardous practice if adopted as a general rule; and that in any case, even of suspected inflammation or threatening congestions, it ought seldom if ever to exceed two ounces at a time.

I know that a spontaneous hæmorrhage from the nose often diminishes the febrile symptoms, but I appeal with confidence to every good observer, to say, if in such cases the quantity of blood lost in this manner exceeds in general five ounces. Nature now and then makes her mistakes as well as the physician, and cases of profuse hæmorrhage from the nose, and now and then, though rarely, from the lungs, or stomach, or instestines, have occurred; but instead of proving salutary, such cases have almost always proved fatal; the powers of life sinking rapidly after such a loss. And so it is with large blood-lettings in typhus, and for the very same reasons; and this is confirmed by the testimony of our very best practical physicians who have written on the disease in an advanced period of their life, that is, when they have been practically taught by long experience.

When an epidemic, or even a sporadic fever assumes the character of synochus, as defined by Cullen, that is, when the brain is not so much affected as in typhus, and the force of the heart not greatly impaired, when the pulse is full as well as quick, when there is much delirium, or even constant and severe headache, with a flushed countenance, intense heat, and general pain, I would not object to the withdrawing from eight to ten ounces of blood from the arm: but I should prefer in

such a case the application of about six leeches to the neck, the bleeding to be encouraged after they fell off, if necessary, by the application of warm poultices of bread and water ; afterwards the applying cold fomentations to the head, together with the internal use of calomel, not in purging doses, but as an alterative, to use an old but sufficiently intelligible word.

When typhus is accompanied with a pain in the chest, (as I observed in a former part of this Commentary,) the typhoid character of the fever is generally to be discovered in the dulness and confused state of the senses ; in such a case, I never take away more than seven ounces of blood at a time, and, in general, I have found even less than this quantity to be quite sufficient as an experiment, especially if followed by the immediate application of a large blister between the shoulders. Fomentations are recommended by some in such cases, but they expose the patient to fresh catarrh.

The same practical observation is applicable to all cases of this kind of fever, when a fixed pain occurs in any part of the abdomen in the first stages; but here I would observe, as indeed I have already done, that active or acute inflammation of the peritoneum, liver, or intestines, even in the first stage, seldom accompanies true typhus. I would say it never does so, did I think myself justified in writing so positively from my own experience alone. Such a complication, however, as this, I have never seen. In the later stages, when congestions and local inflammations frequently occur, they are so decidedly of the asthenic or passive kind, as to forbid the free use of the lancet; but, in truth, they are always to be looked on as very dangerous events. The second remark to which I alluded, is, that we have few pathognomic symptoms by which, in the second or later stages of typhus, we can ascertain with certainty the existence of

inflammation in any of the abdominal viscera. The patient is incapable of expressing his feelings, and his shrinking under forcible pressure of the hand, is a very doubtful proof. But when meteorismus is accompanied by pain on pressure, intestinal inflammation may be suspected to exist. In such cases I have seldom derived so much advantage from blood-letting from the arm, or even from leeches, as from blisters, combined with the judicious administration of calomel in small doses united to the pulv. antimonial Ph. Lond. or ipecacuanha. These are good in visceral inflammation of a slow or passive character, and indeed also in acute inflammation of the liver after appropriate blood-letting; but in the last case, mercurials must be given frequently, as is well known. I appeal with confidence to the experience of able practitioners, if in the last stage of typhus they are deterred by the suspicion of intestinal inflammation from the supplying the patient with wine and nutritious drink, or even of cordials. In no country of Europe does the contagious typhus appear oftener, as an epidemic, than in Ireland, where the low diet, the scanty clothing, the damp and confined habitations, and other concomitants of extreme poverty, abound, and I ask of the many excellent and successful practitioners of that country, if in the second and later stages of such a fever, even in its worst form, when visceral inflammations are said to be present, the chief remedies to be relied on are not more or less of a stimulating and cordial kind. Numerous are the cases of recovery, even of the worst description, where the chief remedies were wine, mineral acids, diluted alcoholic liquors, fermented liquors, soups of meat, the best of which are chicken-broth or beef-tea, not too strong, and perfectly free from grease; arrow-root, and sago with wine; ammonia, confectio aromatica, decoction of serpentaria, camphor, musk, opium, and sulphuric

ether, ; the last of which requires to be combined with other articles of the materia medica *ejusdem generis*, all of which would be injurious if given in the same quantity in acute inflammation.

It appears to me, after having nearly concluded a long life of active practice, that the treatment of continued fever, as it was followed by myself, as well as by many of the pupils of Cullen, was defective in the first stages of typhus by our neglecting to employ emetics so generally as was then common with us.

I have tried to explain the reason why vitiated secretions are formed in the stomach, almost immediately after the invasion of the disease. The symptoms which arise from this source, such as the anorexia, nausea, thirst, and headache, confirm what the doctrine teaches.

Old physicians, of the very highest name as authorities in practice, made it almost a general rule until the time of Cullen, to begin the cure of typhoid fevers with an emetic; and I must confess, after upwards of fifty years acquaintance with the malady, I am disposed to think that the practice has been too much neglected, not only in the commencement, but during the course of the malady.

Emetics are useful in a multitude of cases; but they are also hurtful and unnecessary in several others.

When there is nausea as well as anorexia, and when anorexia is accompanied with oppressive headache, or a tendency to coma, when the tongue is covered with a thick mucous coating, whether it be dry or moist, but more particularly if moist, an emetic is indicated, and is generally useful. There are, however, many cases in which it is unnecessary and even hurtful.

In cases of typhus mitior, where the tongue was not foul, nor the headache great, I have avoided giving it, as a useless and doubtful remedy; also, when the fever

began with high delirium, and when there was evidently much determination of blood to the head.

But when typhus, with prevalent bilious symptoms, rages as an epidemic, owing to the atmospherical character of the year or season, I think the practice of Sydenham, Sir John Pringle, and others, ought to be followed, and emetics to be given more freely than has been customary of late years.

There is a kind of coma, which occurs now and then in typhus, about the seventh, eighth, or ninth day, sometimes later, which is entirely symptomatic of an affection of the stomach. There are no symptoms of congestion of blood towards the head; the pulse is regular though weak; it seldom beats more than 112 per minute, and the patient, when roused, answers the questions put to him, though with a feeble and faltering voice. In this case, an emetic frequently cures the symptoms, and seems to give a favourable character to the course of the disease. The following case will best illustrate the condition of such patients.

In 1802, I was desired by the partner of Mr. D., the apothecary to Queen Charlotte, to visit a young lady, whose case he thought alarming. In was in the seventh day of a case of common typhus mitior, in which no untoward symptom had occurred, until the afternoon of the preceding day, when the patient was observed to become gradually insensible and comatose. Her pulse, when I saw her, beat about 112, and I was informed it had not been quicker at any time except during the two or three first days, when it generally beat about 120 per minute. It was equal, and was full enough. The heat of the extremities and of the whole body was moderate. Her bowels had been relieved by enemata, and occasionally, by means of rhubarb. She had no desire for food, but did not refuse drink. She could be roused for a moment,

but seemed unwilling or unable to speak. She had taken, until the time of my seeing her, the common saline diaphoretic of Riverius, with fifteen drops of antimonial wine in each draught, and she had been supported by barley-water, and occasionally a little wine. It had been feared that either a fatal collapse or an effusion of serum in the brain was taking place rapidly, but against the first of these suspicions, the action of the heart, the state of the respiration, and the expression of the countenance, were sufficient arguments; and against the second, the ease with which the patient could be roused, and the appearance and expression of the eyes when she opened them, rendered the conjecture improbable. I suspected a vitiated secretion of mucus in the stomach to be the cause; and, accordingly, I ordered a common mild emetic of sixteen grains of powdered ipecacuanha root, with orders to give the patient one or two tea-spoonfuls of brandy in a cupful of barley-water, as soon as she was recovered from the fatigue of the emetic. I also changed the medicines, by causing the antimonial wine to be left out of the saline febrifuge, and to supersaturate it with the liquor ammoniæ, so that the patient should take about five drops of it in each dose. I was to have been consulted again the next day if necessary, but I heard no more of my patient until several days afterwards, when I met the apothecary at the house of another patient. He then told me that the emetic had done wonders; for soon after its operation, she began to recover her senses, and called for some drink, and that by persisting in the use of the medicines prescribed, and a little wine with her drink, her convalescence appeared certain.

Another defective part of my practice, as well as that of several other contemporary physicians, consisted in persisting too long in the employment of antimonials. I cannot pretend to say what were the motives with

others, but in regard to myself, I know that my obstinate adhesion to this practice for so many years was entirely owing to my belief in Cullen's proximate cause, namely, a spasmodic constriction of the capillaries, and the supposed necessity of perspiration.

There are few of our febrifuge medicines which weaken the action of the heart and of the stomach more decidedly than antimonials, when too long continued. They prevent the return of natural appetite, by creating a sensation of nausea, which the patients, owing to the state of their intellects, cannot express. After some years of practice, I abandoned the employment of the tartrate of antimony as a diaphoretic in the later stages of typhus, and, indeed, I have seldom given it as an emetic. There is a very old nostrum (but not the worse for being very old, or for being an unfashionable combination, or even a nostrum) which I prefer. It is a scruple of the pulv. ipecacuanhæ, and one drachm of the oxymel or syrup of squills, as an average dose. This is more certain in its operation than tartar emetic; it cleanses the stomach sufficiently from vitiated secretions and undigested food, and does not produce the protracted nausea and great languor which often follow the exhibition of emetic tartar.

The petechiæ and vibiciæ which occur in the second and later stages of typhus gravior, show us plainly what the doctrine teaches, viz. that owing to the weakness of the circulation and accumulation of blood in the arterial system, it is slowly propelled, and becomes loaded with carbon before it reaches the smaller veins; and by the result of Bichât's beautiful inquiry, entitled *Researches on Life and Death*, as well as by those of Legallois on the Principle of Life, we know that venous or carbonised blood weakens and ultimately destroys the energy of the brain and spinal marrow, as well as that of the heart;

consequently, timely remedies for such a state of our fluids ought to be had recourse to. These remedies are not stimulants, or even tonics in the usual sense of such words, they are chemical agents which supply oxygen to the blood, and which act on that principle alone. They are the mineral and vegetable acids, which experience, independently of all reasoning, has proved to us to be of great service under such circumstances. To me they seem to act on the same principle that they do in true scurvy. In both of those diseases they are beneficial, and yet there is no other analogy between such diseases than the carbonization of the blood. It is not, therefore, unreasonable to suppose that acids should act on the same principle in both diseases. Acid fruits, if ripe, are useful in both complaints; especially if wine, or a small quantity of any alcoholic liquor, be added to the acids as a stimulus to the stomach.

I have but few remarks to make on the employment of warm-baths in typhoid fever. But perhaps they may not be deemed altogether unimportant. The temperature of those I employed varied, according to the heat of the patient's skin, from 86° to 100° Fahr.

There are few remedies which require greater sagacity or caution in the administration, than that of a tepid bath in typhous fever; but I suspect this is not generally either felt or acknowledged. In my hands its benefits have in some cases surpassed my expectations; in others which appeared to me to be similar, as far as the natural differences of individual constitutions permit one to judge of similarity in diseases, and in which a bath of the same temperature was employed for the same space of time, and with every necessary precaution, such a profuse perspiration was produced as sank the powers of life of the patient, and terminated in a colliquative sweat.

The objects which I hoped to attain by means of this

remedy were the producing a mild equable and critical diaphoresis, the prevention and relief of visceral inflammation, and also of delirium and congestion of blood in the head. I confess that I find it difficult to describe with requisite accuracy the circumstances of the cases in which it either fully answered or disobeyed these intentions. I should say, in general, that it may be employed with safety and with the hope of success at any time of the fever, between the fifth and the eleventh day of that disease. In truth, it is seldom that a physician has opportunities of ordering this remedy before the fifth day, as his advice is seldom required sooner. When the powers of life are greatly exhausted, which is usually the case after the eleventh day of the fever, there is danger of producing a colliquative sweat, by means of a hot-bath, especially if the patient be kept too long in it. The perspiration breaks out soon after he is placed in his bed, and the practitioner is, at first, deceived by the apparent relief it seems to afford; but in these fatal cases the perspiration is always too copious, and it proceeds rapidly, until the pulse becomes fluttering and weak, the feet and hands cold and clammy. When, on the contrary, the warm-bath excites a gentle, mild, and equable sweat, during which the pulse becomes softer and slower, and the patient falls into a quiet sleep, his convalescence may be looked on as almost certain from that period.

Febrile patients, in general, seem to experience a degree of comfort and relief while in a warm-bath. It allays the subsultus tendinum and nervous twitchings when these are present, but I suspect it is a great error to allow patients to remain too long in the bath, which one is tempted to do from the relief it seems to afford. It was to the long duration of the patient in the bath and his previous weakness, that I attributed the collapse and colliquative sweat which followed its employment.

Therefore I deem twenty minutes to be nearly the extreme time in which the patient ought to remain in the warm water.

Upon taking him out of the bath he ought to be instantly conveyed to his bed, from which the sheets have been previously removed. He may either lie between two fine light woollen blankets, or in fine flannel. The moisture on his skin is then soon converted into vapour, which promotes the perspiration. The patient ought not to be dried when he comes out of the bath.

Whoever orders this remedy for a patient labouring under typhous fever ought to remain with him, not only during the whole time he is in the bath, but for, at least, half an hour afterwards, or else he ought to have the assistance of some medical friend who will perform this duty for him, and on whose judgment he can rely; for it is impossible to give such directions to sick nurses and common attendants as any individual case whatever may require. The state of the pulse and strength of the patient must determine the length of time which he ought to remain in the bath. On coming out of it, and when he is in bed, it is always useful to give him some tepid drink, with a suitable quantity of wine, which is to be repeated according to circumstances. The perspiration must not be too much encouraged. A slight moisture of the skin, which continues without increase or abatement for some hours, is the most desirable effect. A table-spoonful of good sherry or madeira, with half a tea-cupful of barley-water may be given about every half-hour, for some hours after coming out of the bath. The air of the bed-room must not be too warm or close, nor the bed-clothes too many. If the perspiration be too profuse, it is right to check it by means of mineral acids in moderate doses.

If the physician is too much occupied to dedicate the necessary time to his patient, or cannot trust to the judgment and experience of some medical friend to supply his place, he will act prudently not to propose this remedy.

Before I conclude this Commentary, I request the attention of young practitioners to two remedies which remain to be described; one of which has been recommended for a most important functional derangement, while the other is a truly general febrifuge, if ever a remedy deserved that name. The one, is the belladonna, which has been recommended by Dr. Graves of Dublin for the alleviation and cure of a peculiar kind of cerebral excitement, which arises in the later stages of the fever; the other is the febrifuge of the late and still justly-lamented Dr. Currie of Liverpool, namely, the aspersion of cold water.

On the last of these two I am enabled to write from experience; and I have several observations to make concerning it, which I hope will be useful; but I take shame to myself, in confessing that it is only of late that I read Dr. Graves' very interestiing paper on the use of belladonna in fevers, which is to be found in the thirteenth volume of the Dublin Journal of Medical Science, and which I strongly recommend to the perusal both of scientific and practical physicians. The title of his paper does not convey a notion of its importance.

He observes truly, that, in the advanced stages of typhus, when patients are often afflicted with tremor, great restlessness, want of sleep, delirium or coma, and other tokens of cerebral affection, the pupil of the eye is in general either preternaturally dilated or preternaturally contracted; and he brings forward many facts and arguments chiefly derived from observation and practice, to show that these opposite states of the pupil

indicate opposite conditions of brain which require different remedies,—a circumstance which has not sufficiently attracted the attention of medical men.

The contracted pupil is a much more dangerous symptom than the dilated one. On this point Dr. Graves expresses himself thus: 'In fever, with cerebral disease, 'one of the most alarming symptoms is marked con- 'traction of the pupil, and were I called to a case in 'which every other symptom was favourable, but great 'contraction of the pupil present, I would say, that it 'was a case of extreme danger. A tendency to even 'moderate contraction of the pupil is a very dangerous 'symptom in typhus; but a pupil extremely and per- 'manently contracted, or, as it has been called, a *pin-hole* '*pupil*, is a highly-dangerous omen.'

In all cases in which this dangerous symptom arises, opium, and all its preparations, without exception, do harm. The black drop, the acetate and muriate of morphia, are nearly equally hurtful as opium itself; and yet the patients, owing to the want of sleep and great restlessness, seem to require a narcotic remedy. In such cases, belladonna has been found successful by Dr. Graves. The origin of his discovery (for such I consider it to be) is interesting, as it exhibits a right method of reasoning in practice, where the nature of a symptom we are desirous of relieving is as yet of too obscure a nature to be understood.

'Some time ago,' says Dr. Graves, 'I attended, with 'surgeon Whyte and Dr. Corrigan, a gentleman of sound 'constitution, labouring under typhus. During the course 'of his fever his head became greatly engaged, and he 'had remarkable contraction of the pupil. We pre- 'scribed tartar emetic with opium without effect, and we 'had the chagrin of seeing a man of vigorous frame, and 'in the prime of life, carried off in spite of all our efforts.

'I was grieved at the result, and talked over the matter 'with Dr. Corrigan and Mr. Whyte. During the course 'of our conversation Dr. Corrigan threw out the sugges- 'tion, that in such cases narcotics which produce dila- 'tation of the pupil might be beneficial.' This observation appearing important to Dr. Graves, he determined to act on it.

The first case in which he administered it was that of a lady rather advanced in years, to whom he was called about the tenth day of the fever. She was at that time much agitated, sleepless, and unconscious of what was passing around her. The pupils of her eyes were greatly contracted. He prescribed as follows:

℞. Ext. belladonnæ gr. j.
Ext. hyosciami gr. vj.
Pil. hydrargyr. ℈j. ft. pil. vj.

One of which was to be given every three hours.

After the third pill she became quieter, and the tremors were less violent. After the fourth she fell asleep, awaking at intervals during the night. The following morning she was more composed, and her pupils were more dilated. As her pulse was still weak and small, she was allowed a little wine; and chicken-broth was given more freely. The pills were continued. Her bowels were kept open by enemata of inf. sennæ with Epsom salts and tinct. of jalap. A few days after, her convalescence was established.

The second case which is related by Dr. Graves is still more remarkable. Independently of its testimony in favour of belladonna for that diseased state of brain which is characterised by great contraction of the pupil, it exhibits the judicious practice which is almost universally followed by Irish physicians in the later stages of typhus, whose opportunities of seeing that disease in all its most dangerous features is certainly greater than in most

other countries. The patient was a man of full plethoric habit, but of a nervous melancholy temperament. On the ninth day of a typhous fever, which had begun with the usual symptoms, he complained of pain in the fore part of the head, for which leeches, aperients, and cold affusion, were made use of, and which relieved the symptom. On the eleventh day his skin became maculated. On the twelth day Dr. Graves was called into consultation by Dr. O'Reilly, the daily attendant physician and narrator of the case. On that day the skin was covered with dark-coloured maculæ, the countenance expressed anxiety, the tongue was white, the pulse 130 per minute; there was great restlessness and want of sleep. On the sixteenth day of the disease the patient was attacked with diarrhœa and great prostration of strength. Dr. O'Reilly ordered him the acetate of morphia with camphorated jalap. This checked the diarrhœa, but the febrile symptoms continued, and were followed by tumefaction of the abdomen, suffusion of the eyes, a very quick pulse, and hurried respiration. On the seventeenth day, when Dr. Graves was called in a second time, all these bad symptoms were aggravated, and the pupils of the eyes were observed to be contracted. He ordered the following draught to be taken every sixth hour:

℞. Moschi gr. x.
Ext. belladonnæ gr. j.
Mucilag. acaciæ ʒij.
Mist. camphor. ℥ss.
Syr. auranti. ʒij. M.

On the nineteenth day the symptoms were nearly the same, but as the diarrhœa recurred, the cretaceous mixture was ordered, also port wine, and occasionally coffee. This checked the diarrhœa, and by persisting in the use of the belladonna, and varying the other remedies and the diet according to the symptoms, a state of convales-

cence began on the twenty-third day, after which his recovery went on progressively. Other cases, relating to the efficacy of the belladonna, are narrated by Dr. Graves in this interesting paper.

It is under the conviction of the necessity of enlarging our experience concerning one of the most efficacious of all anti-febrile remedies which modern practice has hitherto discovered, that I am induced to offer the following observations, with which I shall conclude this Commentary. They relate to the affusion of cold water over the whole surface of the body, or the immersion of the body in a cold-bath during the first stages of typhus.

This remedy is the only one I am acquainted with, which has a decided power of arresting typhus in the beginning; but the dread of congestion and of visceral inflammation which has seized the imagination of so many physicians lately, must naturally prevent its being employed as often as I think it ought to be.

I am convinced it will still be a long time before this truly valuable remedy comes into general use, except it be perhaps in camps, fleets, and hospitals, where the reputation of physicians and surgeons are less dependent on the opinion of the public, and, may I add, without offence, where they are less exposed to suffer from the malevolent remarks and insidious interpretations of rivals, than either court or popular physicians are, whose reputation is often so seriously affected by designing as well as by thoughtless *commerage*.

With Dr. William Wright, to whom British physicians first owed the knowledge of the efficacy of the affusion of cold water in fevers, I had the happiness of being intimately acquainted during a number of years. A man of greater modesty, candour, and moral rectitude, I have seldom known. He was incapable of recommending anything new in practice, either from hypothetical

opinions or the unjustifiable desire of a momentary reputation. He had seen much practice, especially in the island of Jamaica, where he had acquired the reputation of a safe and successful practical physician. I do not assert that he was the first who employed the affusion of cold water in fevers. In the Eph. Nat. Curios. decad. iii. Ann. iii. Obs. 48, there is a case of a cure of fever by means of a cold bath, and it seems probable that Celsus also employed this remedy in some febrile disorders; but until the publication of Dr. Wright's essay in the seventh volume of the London Medical Journal, it was unknown to British physicians. This essay, I suspect, would have failed to attract the attention it deserved, had it not been for the genius and philosophical turn of mind of another Scotch physician of great eminence, I mean Dr. Currie of Liverpool; who soon began to try it on an extensive scale; and he has given to the medical public the result of these experiments in his well-known and admirable work, *Medical Reports on the Effects of Water, Cold and Warm, as a Remedy in Fever and other Diseases.*

The employment of cold water externally in continued fever requires a nice discriminating faculty on the part of the practitioner; and he who intends to try it for the first time, would do well to imprint on his memory the chief admonitions (not all of them) of Dr. Currie, as well as the result of his cases.

The great guiding rules for its administration may be concisely expressed.

1st. When the heat of the skin of the patient is steadily much hotter than in health, and that there is no fixed pain in any part of the chest or abdomen, nor catarrhal symptoms, the sudden affusion of cold water, or the immersion of the body into the same, may be employed with perfect safety, and, according to my limited experience of its action, with success.

2nd. Neither delirium, nor incipient coma, nor a dry tongue, nor costiveness, nor suppressed secretions or excretions of any kind, are to be considered as sufficient counter-indications.

3rd. The greater the heat of the body, and the quicker and stronger the pulse, so much greater are both the safety and efficacy of the remedy.

4th. It must not be tried in any state of collapse, as indicated by a very feeble pulse, cold extremities, cold, partial, and clammy perspiration. In all such cases it is a truly dangerous agent, which may prove fatal in a few hours after the application.

5th. I consider it to be hazardous, when there is diarrhœa or frequent scanty watery stools. But as I have never ventured to employ it in such cases, I cannot speak positively as to its effects in them.

6th. Both the aspersion and the immersion ought to be sudden and of short duration.

7th. They may, and, indeed, must be repeated in many cases at intervals, according to symptoms, which I shall attempt to describe afterwards.

The following case, in which there were all the external characters, which in the present days, are often supposed to denote cerebral inflammation in the first stage of typhus, and in which previous blood-letting had not been employed, will serve to introduce other remarks I have to make on this important part of practice.

A very active and vigorous man, about thirty years of age, by profession a fencing-master, was, after great bodily fatigue, exposed to a cold and very damp atmosphere one evening towards the end of autumn. He complained much, as I was afterwards informed, of general pain, listlessness, alternate heat and rigors, great pain all over him, and headache. During the night he was seized with a regular shivering fit, followed by great

heat of skin, dryness of the mouth, a very quick pulse, thirst. These symptoms and the headache continued to increase without any intermission, until delirium ensued, which took place in the evening of the second day; but his medical attendant, being struck with his faltering speech, and imperfect pronunciation of words, and the general confusion of his intellects, and knowing that typhus raged at the time, did not apply even a few leeches to the neck to relieve the head. I saw him for the first time on the fifth day of the disease; he was then completely delirious, his eyes red and suffused, his countenance was flushed, his pulse beat 120 per minute and strongly, the skin was very hot and dry, and the urine a little high-coloured, but not depositing any sediment. His bowels had been kept open by a mild aperient. I resolved on trying the cold-bath with this patient; and accordingly, ordered a bathing-tub with cold water, of about sixty degrees of temperature according to Fahrenheit's thermometer, to be placed at the side of his bed; and on the other side of it, a couch with a mattrass, having a couple of soft woollen blankets, (without a sheet,) ready to receive him. His shirt was taken off, and he was then raised up by four assistants in the sheet on which he lay, and gradually, but rather quickly, immersed in the cold water. He uttered a loud shriek on the first impression of the water. I did not allow him to remain above a minute in it, and after plunging him a second time in the water, removed him to the couch, covering him well, and surrounding him with the blankets, and not allowing the attendants to dry him, being well aware that the heat of his body would soon convert the moisture into vapour, which could not do him any harm. His pulse lost about twenty pulsations per minute, and he remained perfectly quiet; but whether he was in a state of stupor or sleep, I could not make

out at first. I caused him to swallow some spoonfuls of tepid barley-water with two table-spoonfuls of *vin de sauterne*, and having desired that the same should be repeated every hour, except he became hot and agitated, I left him. I was informed on my return, six hours after, that he had been perfectly tranquil; but that he did not appear to be conscious of what was passing around him. He was in a mild sweat, his pulse had risen to 110, but was soft and equal. There was here no necessity to repeat the cold-bath, nor even for any medicine, except small quantities of drink. He passed a quiet night, but was still confused and affected with low delirium. Towards morning he fell into a state of more natural sleep, and from that time his recovery was rapid; so that at the end of the tenth day his senses and natural sleep returned, and he daily gained strength without the intervention of any tonic or stimulating remedy, except mild nutriment, and occasionally small quantities of wine. I have often employed immersion in cold water in the first stages of typhus, but not so often in private practice as I could have wished, having had to combat the fears of relatives, and too often the apprehensions of learned brethren; and at the time I allude to, now more than thirty years ago, neither my age nor experience gave me the necessary authority to enforce my opinion, nor the courage to risk my reputation in opposition to the advice of others.

It would swell the Commentary unnecessarily were I to detail the cases in which I have employed this powerful and excellent remedy, and, indeed, I can add but little useful information to the numerous valuable observations of Dr. Currie. Still I am desirous of offering a few admonitions on the subject, which I think may be useful.

I prefer much the immersion of the patient into cold water to the aspersion of his body, as recommended by

Drs. Wright and Currie, for the following reasons. In order to asperge the body, the patient must either be held upright or seated upright; for the sponging him in a horizontal posture does not produce a sudden and general effect like immersion and aspersion. But when we attempt to make a patient stand upright who has been labouring some days under the influence of typhus, he is apt to be immediately seized with faintness, he grows giddy and pale, and his pulse sinks suddenly, both in quickness and strength, so weak is the real action of the heart; and this state of lipothymia is, as I have already remarked, a dangerous one for the application of cold water in any way. Better never to have dreamt of the remedy, than to try it in such a state of weakness of brain and circulation of the blood, for if very cold water be now thrown over a patient in such a state, it is very doubtful if sufficient reaction occurs afterwards. On the contrary, the brain is apt to become more paralyzed than before, and a dangerous collapse follows.

I attribute many of the unsuccessful trials with Dr. Currie's favourite mode of aspersion to this circumstance. Besides, the patient is exhausted by the exertion of standing upright, though supported by attendants, and on the whole he is too long exposed to cold. I know it will be said, that on the very first trial which Dr. W. Wright caused to be made on himself, he was in an upright posture, but he was in a warm climate, and from the minute directions he gave concerning the trial to be made with himself, his sensorium could not have been much weakened by the fever. The aspersion and upright posture do not possess one advantage over immersion which I am conscious of.

Dr. Currie attributes much virtue to sea-water or to salt and water. I have never employed either; and am satisfied that it is the temperature which does all the

good; though I confess that, were I residing at a sea-port, as Dr. Currie was, I should have preferred it, and yet I can assign no good reason for such preference, except it be the slavishness of one's understanding to a commonly-received and unexamined prejudice.

The immersion may be employed at any period of typhus, provided the skin be hot and dry, and the pulse moderately full; but it is chiefly useful in the early stages of the disorder, before the patient is too much exhausted, and probably before local inflammations happen.

The greater the heat, the drier the skin, and the fuller the pulse, the greater is the success. I dare not take it upon me to recommend it when there are pleuritic pains, or even slight catarrhal symptoms. I dread hazardous trials of all kinds with human life; preferring to be called a timid practitioner, rather than a presumptuously bold one. I know that Dr. Currie has tried the cold aspersion under such circumstances, and, as he assures us, with success; but I never could make up my mind to imitate his example in such cases.

When the pulse is feeble, small, and quick, which denote that the heart is too weak to expel its contents at each contraction; if there be coldness in the extremities or a clammy sweat on the skin, I deem both the immersion and aspersion dangerous. This powerful and excellent remedy is not to be tried in general without great circumspection, after the tenth or eleventh day of the fever.

The result of Dr. Currie's extended experience of five years with the remedy, is thus expressed by himself, in the second edition of his work:

'I repeat that when used in the first three days of 'fever, the cold affusion generally stops the disease, that 'the same happy effects sometimes follow its use on the 'fourth or fifth day, but seldom later; that even in the 'subsequent stages, when the heat continues preterna-

'turally great and the skin dry, it is of great and mani-
'fest advantage, almost immediately relieving the most
'distressing symptoms, particularly restlessness and deli-
'rium, and conducting the disease to a safer and speedier
'issue.'—Vol. ii. p. 325.

When the immersion in cold water reduces the quickness of the pulse from ten to twelve pulsations per minute, a concomitant diminution of the heat of the skin generally follows, together with a great relief from restlessness and subsultus tendinum. After an hour or two, sometimes sooner, sometimes later, the pulse begins to increase in quickness. This symptom, combined with an increased heat and dryness of skin, must determine the practitioner as to the necessity of repeating the immersion. It is not a remedy to be employed at fixed intervals, but solely *pro re nata*.

If the pulse do not rise more than ten pulsations per minute in the course of twelve hours after its depression by immersion, it is a very favourable symptom, and announces the commencement of a generally favourable change in the condition of the patient. Should the skin remain soft and gently moist, the repetition of the cold-bath becomes unnecessary. Mild diluting, yet nutritious drinks of a vegetable kind, such as barley-water, diluted tapioca, also chicken-broth or beef-tea, and small quantities of wine, given every two or three hours, and the common saline diaphoretics, are in general all the medicines which such a patient requires, until the brain, and the secretory and digestive organs, begin to recover their functions.

Such cases, however, are not general; on the contrary, in the greater number, the abatement of febrile action after the first cold-bath, rarely continues many hours. It is then apt to return with as much violence as at first, the pulse augmenting in quickness, and the skin

becoming again burning hot and dry. Here there must not be any hesitation on the part of the practitioner. The immersion must be repeated as such events dictate. I request it to be understood that I limit my opinion of the necessity of such a rule and its beneficial effects, to the first stages of the fever, and I beg to repeat, that neither violent headache, delirium, redness of the eyes, or suffusion of the countenance, are to be considered as true indications of cerebral inflammation, or as counter-indications for the employment of this remedy.

Dr. Currie, whose trials with the affusion of cold water in fevers became very numerous after the publication of the first edition of his work, mentions cases in which it produced the happiest effects, even in very advanced stages of the disease, as I have already remarked. Among others, he mentions that of a servant of a Mrs. Heywood, daughter of his friend Mr. Percival. 'I used it,' he says, 'in the fourteenth day of fever, 'with the immediate removal of delirium, and every other 'desired effect.' Such trials I have not made; but the following case, related by Dr. Currie, is well worthy of recital and attention, proving the beneficial effects of cold generally in typhous fever.

'In the month of May, I was desired to visit a patient 'ill of fever in Sparling Street, Liverpool. I found him 'in the tenth or eleventh day of the disease, delirious, 'and restless; the surface of his body dry, and the heat 'of his body 104 Fahr. The air of the room was close, 'and I desired the only window in it to be opened. The 'wind from the north-west blew directly into this window, 'and the bed being situated between it and the chimney, 'a pretty brisk stream of air passed over it. The pa-'tient had just thrown off a part of his bed-clothes, and 'was exposed almost naked to the wind. I sat by him 'with my finger on his pulse, watching the effects. In

'a little while the pulse fell from 120 to 114 in the 'minute. He became more tranquil, and soon afterwards 'sank into a quiet sleep, in which he remained while the 'water for effusion was prepared; of course we did not 'disturb him. When I left him, I desired the attendants 'to suffer him to remain in this situation all night, unless 'he became cold; but to take care to administer the 'proper nourishment. Once or twice in the night the 'attendants placed the bed-clothes on him, but he soon 'became hot, and they took them off again. While naked 'he slept tranquilly, and had generally a gentle moisture 'on the skin. In the morning I found him perfectly col-'lected, his pulse about 100, and his heat 101° Fahr. 'He coughed a little, and we covered him with a sheet, 'which he now found agreeable to his feelings. The 'cough produced no serious inconvenience, and in a few 'days the patient recovered under the common treat-'ment.' At the time of this experiment the mercury in the thermometer stood at 65° Fahr., and it did not sink under 63° in the night. Its relation to the effects of cold water must be so obvious as to render any comment useless.

This case is of great value, if it be associated in the mind of the student with all the other facts that relate to the beneficial action of a constant supply of cool and fresh air to patients who labour under any variety of ataxic or adynamic fevers, whether they belong to typhus, to small-pox, or to the scarlatina anginosa gravior. The action of cold air (a current of which I do not recommend though I do the free admission of it) on the pulse of the patient coincides with the effects of the affusion of cold water, and of the immersion into a bath of the same cold fluid. Its effects in calming his agitation and restlessness are no less remarkable, and it must, I think, recal to the memory of every observing practitioner, the

perpetual movements and tossing about of the legs and arms of febrile patients when too much covered with bed-clothes, or when the air of the apartment is hot and close.

If it be asked how the salutary operation of the cold aspersion or bathing is to be explained by the doctrine I have advanced, I might answer that, in giving my testimony concerning it, the difficulties of reconciling it to the hypothesis never once presented itself to my mind. Were it wholly inexplicable, that circumstance would not invalidate the testimony of such men as Dr. W. Wright or Dr. Currie, and I hope I might, therefore, plead the same advantage in regard to mine. But the truth is, after due reflection, it appears to me that there is little difficulty in explaining the facts by the hypothesis.

A great portion of heat is rapidly withdrawn from the surface of the body, and consequently, the temperature of the whole mass of blood soon suffers a diminution. Its volume is of course much diminished ; and hence, as is proved by Schwann's experiments, the contractility or elasticity of the arteries is increased, so that the plethoric or congestive state of the whole arterial system is relieved as effectually as if a quantity of the circulating fluids had been withdrawn ; but with this superior advantage, that no vitality is lost, as is the case in venæsection.

The diminution of the volume of the circulating fluids by the condensing power of cold on the one hand, and the increased pressure on them by the contraction of the arteries on the other, promote the propulsion of the blood, which is immediately followed by all the advantages which attend a freer circulation.

As the object I had in view in writing this Commentary was to point out the bad effects of such doctrines as produce sudden innovations in practice relative to adynamic fevers, which are contrary to the almost

generally-acknowledged results of past experience, and having attempted to the best of my abilities to reconcile the results of experience or of rational empyricism with the conclusions which a legitimate study of the symptoms appear to justify, I take leave of it, although I am fully aware that many important points of practice remain unnoticed, — such, for instance, as sponging the body with tepid vinegar and water, or a mixture of salt, vinegar, brandy, and water, in the later periods of the disease, and the treatment of symptoms which call for special remedies, such as opium, musk, and camphor, &c. But so much has been written by many able practical authors on these subjects, that I deem any observations of mine superfluous. Still I cannot conclude this essay without expressing the hope that multiplied trials on a large scale may be made with the immersion of the body in cold water, or the asperging it with the same, after the manner of Wright and Currie, for we are in need of a great number of trials, in order to beget that confidence in the remedy, which I think it deserves.

There are some important points regarding the employment of cold water externally in fever, which repeated trials and attentive observation can settle. These are, the best degree of temperature, the length of time that a patient ought to remain in the cold-bath, and its repetition. By the expression *best degree of temperature*, I mean that temperature of the water which diminishes the production of febrile heat the most effectually, for the greatest length of time, and with safety to the patient. But it is clear that, although a given degree of heat, say fifty or sixty degrees of Fahrenheit, may be found to be the best and safest on the average, still individual cases will demand modification of the general rule; and the same observation must hold good as to the length of the intervals to be observed before either the immersion

or aspersion is to be repeated. It is only in military and naval hospitals where such experiments can be made with any hope of useful information, for in them alone is the opportunity offered to well-informed physicians and surgeons of seeing the disease in all its stages and varieties from the very beginning to its termination.

It will be said by some, that the cure of fever, as well as of all dangerous diseases, depends more on the powers which are inherent in, and peculiar to, the individual constitution, than on the medical treatment; for if faith is to be put in the testimony of medical men of equal reputation, remedies which are the most opposed to each other have produced similar beneficial effects. Can any two methods of treating typhus fever, be more opposed to each other than those of Dr. John Clarke of Newcastle, and of the late Dr. Armstrong of the Fever Hospital in London? And yet each of these gentlemen, with their imitators and abettors, write with equal confidence concerning the results of their experience.

In such a case how is the young physician to act? If other physicians than the authors of such new doctrines and innovations in practice do not settle the question by means of repeated trials, he must indeed try them himself, and in doing so, he ought to proceed, not only with great caution, but with an impartial mind. He must dismiss from it all prejudices and pre-conceived opinions; and if he do so, it seems to me incomprehensible that he should fail to arrive at a just conclusion; for although strength of constitution, the nature of which is not well understood, may enable a few to resist the action of dangerous agents, they seldom fail to excite symptoms which announce great disturbance, and consequently, to one who is well acquainted with the general phenomena of health, it seems impossible that he should not be able to distinguish the beneficial from the hurtful effects of a

medicament or a newly-imagined plan of treatment. If, for instance, in a bilious fever, the evacuation of bile by emetics and well-contrived purgatives, relieve every symptom, the beneficial effect of the remedies cannot surely be doubted; and on the contrary, if such evacuating remedies be omitted for some days, at the end of which there is a return (*récrudescence*) of all the bad symptoms, together with some new ones of an untoward character, and that, instead of acting on the liver and intestines by appropriate remedies, large quantities of blood be now withdrawn, which treatment is followed by delirium, a greater yellowness of skin, and more disorder in the system, he must, indeed, see through another medium than that of an unprejudiced mind, if he does not conclude that the emetic and purgative plan is good and safe practice, and the venæsection system bad and dangerous treatment, though the last may agree better with a doctrine. If in the first stage of typhus he tries Dr. Clarke's method, and gives two ounces of a strong infusion of cinchona in cinnamon-water, and is informed that soon afterwards the patient became very hot, agitated, and uneasy, that he slept badly, complained much of headache, and had great thirst, that he had frequent convulsive twitchings in his muscles, and finally grew delirious, he would certainly be justified in presuming that the medicine produced these effects, although he had been taught to believe the disease was one of debility.

If circumstances prevent a young practitioner from resorting to a remedy, which, as far as human experience yet goes, has the power of subduing febrile action in its commencement, I mean the immersion of the patient in cold water, the rules for the administering of which must be well studied, and the effects carefully watched, he will then act prudently by following the plan which

the experience of centuries has approved, rather than by trying dangerous experiments which are derived from post mortem appearances, and new doctrines, however ingeniously devised or confidently recommended.

It has been the great object of this Commentary, to place these opinions in a clear point of view, and to prove, by a reference to the testimony of celebrated physicians, and also, by endeavouring to throw light on the nature of fever, by legitimate physiological and pathological reasoning, that the phenomena of fever, and the appearances after death, may be perfectly reconciled with established practice.

COMMENTARY THIRD.

ON SOME IMPORTANT QUESTIONS RELATING TO INSANITY BOTH IN A MEDICAL AND LEGAL POINT OF VIEW.

Of the reasons which render it difficult to give a good definition of insanity—Different sense in which it is considered by the two professions of law and medicine—The extensive sense in which it is considered by the first—The restricted one in which it is considered by the latter—Concise classification of the thoughts which harass the insane—The circumstances which distinguish true insanity from all other diseases—The author's definition of insanity—Of mania—Its premonitory symptoms when it arises from the influence of inheritance—Cerebral excitement described—Of other causes beside inheritance which produce this affection of the brain—In none of its forms ought insanity to be considered as a disease of the mind or soul—It is in all instances an organic affection—Seat of mania—Great merits of Foville and Delaye in discovering it—In what it consists—On blood-letting and the effects of some other medicaments in mania.

It will, I presume, be admitted by every competent judge, that there is not a mental manifestation which belongs exclusively to insanity. Numerous are the disorders, both acute and chronic, in which the brain is so greatly deranged, as to prevent the impressions of external objects on the senses, from producing their usual effects on the mind, (commonly called perceptions or mental perceptions,) or of permitting the functions on which comparison, reflection, and judgment depend, to be freely exercised.

Delirium, and all kinds of hallucination, the manifesta-

tions of which are exactly similar to what we witness in insanity, (idiocy, and imbecility of intellect excepted,) arise from fevers of all kinds, from hysteria, or from uterine, intestinal, or hepatic irritation, or from the sudden translation of certain diseases, as well as from the action of some poisons. They may also be produced by great loss of blood, by depressing passions, by large doses of narcotic and sedative remedies, and, as is well known to medical men, by excessive venery, especially in old people.

In every case of disordered intellect which does not fall within the true character of insanity, we discover decisive symptoms of other disorders, which either precede or accompany the aberration of reason, and which account for the mental phenomena; whereas, in true insanity, let its variety be what it may, even acute mania, in which the whole faculties of the mind are in a state of great confusion, there are no corporeal symptoms to which the psychological phenomena can be referred; for, although it cannot be denied that in cases either of acute mania or of maniacal melancholy some symptoms of bodily disorder may at times be discovered, yet these are so disproportionate to the mental affection, and are so uncertain and variable in their appearance, that they cannot be considered as essentially connected with the malaldy.

True insanity is indeed sometimes associated with other disorders, such as palsy, epilepsy, and catalepsy, but it is also a fact that these are not necessarily connected like cause and effect, for insanity often exists for a great number of years, consecutively, in individuals who have never been either paralytic, epileptic, or cataleptic; and, on the other hand, these diseases often occur and continue for years without insanity. When, therefore, insanity is found to be associated with them, the fair conclusion is, that they do not stand in the necessary

relationship of cause and effect. They are accidental concurrences of two distinct diseases. It may happen that some cerebral affections of brain, such as tumor, may produce both palsy and insanity, but it does not follow that the cerebral affection which produces mania must of necessity occasion palsy. In cases of symptomatic delirium or hallucinations, bodily disease generally points out the true cause of the mental infirmity. But in certain cases of insanity, for instance, in monomania, even that of the most dangerous character, there is frequently such an appearance of health and rationality as often imposes both on the learned and unlearned; and this is the reason why an individual who labours under its influence is allowed his liberty and the possession of every civil right, to the danger of society, the ruin of his fortune, and the unhappiness of his nearest relatives, until peradventure some overt act declares the true state of his brain.

These observations, which might be greatly multiplied were it necessary, appear to make it desirable that a good definition of insanity should be formed, which would be equally useful for purposes of law as for medicine; and yet, when we reflect on the logical character of a definition, and the nature of the malady, and the different sense in which it is considered by the two professions of law and medicine, we cannot avoid coming to the conclusion, that such a problem is a difficult one to solve in a manner which shall give general satisfaction, and yet there is scarcely a writer on the disease who has not attempted the task.

Visible objects, however complicated their parts may be, are susceptible of being accurately defined by any good observer, who is master of language suited to the subject. Many diseases also are capable of a tolerably satisfactory and concise definition, because their diagnostic

characters consist of assemblages of external and evident symptoms. But this is not the case with aberration of reason, for there is not any assemblage of psychological signs which are peculiar to it alone, and the cerebral affection which produces it cannot be discovered by sight until after death, and not even always then.

In all litigated questions relating to the soundness of judgment of an individual, the sole object of the law is to ascertain if he was *compos mentis* at the time he committed a certain action, no matter whether it were criminal or merely the signing of a contract or of a will, &c.; and consequently, this gives a latitude to the legal meaning of lunacy or unsoundness of mind, which cannot be reconciled to the more restricted sense in which medical men employ the same word.

A man is deemed *non compos mentis* in a legal sense, not only when labouring under insanity, properly so called, but when his judgment is impaired to a certain extent by some natural infirmity, such as abolition of memory or mental imbecility from old age, the sole object of the law being to ascertain if the individual could exercise a sound judgment at the time he committed or performed a certain act.

Insanity in a medical sense, whatever its variety may be, is justly considered by many as a distinct and idiopathic disorder, and on this account it might be supposed to be capable of being defined so as to distinguish it from other diseases which have some resemblance to it; and yet, when we compare the definitions which have been given of insanity by the most eminent writers on the subject, we discover such a discrepancy in them as proves that there must have been want of correctness in the notions which the authors of such definitions entertained concerning it.

Dr. Conolly, after giving a very graphic description

of the disordered imagination of an inmate of a lunatic asylum, who laboured under chronic mania, (see his Indications of Insanity, p. 287,) observes that "the features of this case were plainly enough distinguishable, but the question continually presented itself, in what did the patient's mind differ from a sound mind? It was thus I found," he says, "the accepted definitions and prevalent opinions of medical authors were of little use in such an inquiry." And Dr. Good remarks, acutely enough, after commenting on Dr. Cullen's definition, and that of some others, "that some are so circumscribed and narrow, that if the inhabitants of Bedlam or of the Saltpetriere were to be tested by them, the half of the inmates would have a right to their liberty, while other definitions are so loose and so little descriptive of true lunacy, as would entitle one half of the world to put the other half in strait-waistcoats."

I will not swell this essay by introducing the numerous definitions of insanity which are given by different authors, but I shall at once endeavour to explain the grounds on which I have attempted to construct a new one for medical purposes.

The thoughts which harass an insane person are naturally, and indeed necessarily, of two classes. The first comprehends modifications of former impressions, and images of external objects. Sometimes they are single, in other cases they are combined with the conception of other objects in irregular and strange grouping, just as in dreams. The second comprehends all imperfect conclusions and acts of thought which are mistaken for abstract truths, and are acted upon accordingly. The first class of imaginations is more common in insane delirium, the second class chiefly afflicts the monomaniac. The mental images which haunt the maniac may represent persons, animals, and all external objects, also

voices and sounds of all kinds; smells, tastes, feelings of pleasure or pain, both corporeal and mental, and inasmuch as each of these naturally begets some desire or aversion, each of which excites its appropriate emotion, the language and conduct of the individual naturally express these, consequently the manifestations of insanity become infinitely diversified. Thus, constant shrieks of horror, dread, and terror, and indeed of perfect rage and despair, are uttered almost incessantly by the unhappy maniac who imagines he sees nothing but beasts of prey or poisonous serpents, or fire, or precipices, surrounding him. Hence, also, the murderous propensity of another, whose mental vision is that of an implacable enemy, whom he sees in every one who approaches him, and whom he tries to stab or destroy, to save his own life. Hence the suicidal act also of a monomaniac who wishes to escape from the ideal, but insupportable torments of his conscience, or from the dread of destitution, or from the still more singular, though very common hallucination, that he is to obtain the honours of martyrdom and the enjoyment of immortality by the sacrifice of his life. Hence, also, veneration, love, vanity, hatred, ambition, and every variety of human passion, may agitate the madman according to the character of the imaginary objects or feelings which provoke the emotions.

True insanity is an idiopathic malady, which is distinguished from every other aberration of reason, by two circumstances. The first is the absence of the physical derangements of the body, which accompany any and every variety of merely symptomatic alienation of mind; the second is, that the lunatic being fully awake can direct his will.

When the external world is shut out, as it were, from consciousness by sleep, the state of one who dreams is so

far like that of the madman, that he is agitated by thoughts which arise from sensations of which he is not aware, but as volition is suppressed by sleep, the phantoms, the emotions, (desires and aversions,) fail to produce any bodily movements or action, while the same condition of mind in the insane person, he being awake, and having the use both of his external senses and will, directs his actions with full energy. In a few instances, indeed, as in night-mare, (incubus,) the dreamer has a kind of consciousness of the efforts he makes to rouse himself, but this very consciousness proves how completely sleep deprives him of the exercise of volition. He feels that he is powerless, as if spell-bound, and his mental distress is expressed by indistinct groans and murmurs, and by very imperfect muscular movements. But there is a state of imperfect sleep, in which the dreamer has not only a certain command of volition, but even the use of his external senses, though consciousness and the reasoning faculties are suppressed, as in the disease called somnambulism.

Considering, then, that the merely psychological character of insanity is similar to that of one who dreams, and that the pathognomonic distinction arises from the absence of such bodily disorder as accompanies all symptomatic alienation of mind, it appears to me that the following concise description might form a tolerably good definition for medical purposes.

Insanity is a disease of the brain, which causes the patient, while awake, to mistake the phantoms and operations of imagination for realities, which, consequently, become the motives of his discourse and actions, while at the time there is an absence of any bodily disorder that can account for the phenomena.

At the risk of being accused of some prolixity, I must offer a few remarks on the first proposition of this definition.

The fact is, that the explaining and establishing it are absolutely necessary for the elucidation of the disease, especially mania, to which I shall for the present confine my observations.

Mania is a disease of the brain. The symptom which frequently announces an impending paroxysm of mania, and which is chiefly to be observed in those who inherit a strong disposition to the disease from their parents, is an affection of a very remarkable kind, both in its origin, its progress, and its truly obscure character. It may properly be called *cerebral excitement.* The patient begins to lose his natural sleep, and to be much agitated, though he will not allow that he has any ailment. The faculties of his mind, which were perhaps of a very common or even dull character in his usual state of health, gradually acquire a degree of vivacity and brilliancy, which astonishes his relatives and friends, and his muscular energy is equally increased and excited. He rises early, and passes rapidly from one mode of employment to another; and his conduct, according to his station in life and his command of fortune, is singularly varied, but is always in contrast to what it was before the excitement commenced. Still there is not a word of his discourse misplaced, nor anything to prove that he labours as yet under any hallucination. His repartees are quick, and often appropriate; if he is a scholar, he abounds in quotations; if he be a man of wealth, there are no bounds to his purchases, largesses, and waste of fortune. He mounts his horse early in the morning, and rides violently for hours, without a motive. When he returns home he gives orders and counter-orders in quick succession, is alternately passionate, proud, overbearing, compassionate, and forgiving. At last the whole of his mental faculties become confused, he talks incoherently, and the maniacal delirium is established.

The cerebral excitement which I have described, is far from being constantly met with as a preliminary affection. It is only among the descendants of insane parents that it is common.

When a disposition to the disease is gradually created in other individuals from accidental causes, such as mental anxiety, plethora, and passionate emotions of mind, hepatic, uterine, or intestinal irritation, the preliminary affections of the brain are, for the most part, a sense of lightness, and of other uneasy feelings in the head, which the patients always say are indescribable, and which often produce apprehensions in the minds of such unhappy persons of the impending malady. Some of these individuals not only give warning of what is about to happen, but insist on their being conveyed to an asylum or placed under proper restraint. Now, this is a feature which does not belong to the first class, in whom cerebral excitement forms a preliminary symptom. They, on the contrary, cannot be induced to believe that they are indisposed, and, consequently, precautions are neglected.

Some patients, previously to an attack of mania, do not refer the uneasy feelings, which they experience for some time before a paroxysm of mania, to the head, but to other organs of the body, especially to the stomach, liver, and intestines, and in women to the uterus, which is often in a state of great irritation; others lose their appetite and acquire a passionate fondness for strong drinks. Flatulency and oppression at the pit of the stomach, cold feet and hands, or burning heat in them, and clammy perspiration, are to be counted among the precursors of mania which are not constant.

But there is one preliminary symptom of insanity, in general, which is common to all the foregoing classes, namely, an almost complete change of moral character.

Loquacity and irascibility of temper are common to nearly all of them. They also lose the affections of love, friendship, and parental kindness. Some become morose and suspicious; others perfectly indifferent. They are also generally impatient of restraint, and even of being questioned concerning their affairs or conduct.

In some individuals these symptoms increase slowly, and continue for many months before the paroxysm of mania is fully formed. In others they advance rapidly, as may be naturally imagined, where the exciting causes are more or less powerful. In such cases the delirium may even be sudden, as sometimes happens when deep grief, amounting to despair, arises from some great and unexpected misfortune, or from sudden terror, or in consequence of physical causes, such as blows on the head and concussion of the brain, or from sudden suppression of habitual discharges, or the translation of exanthematous inflammation.

To imagine that insanity is a disease of the mind or soul, though maintained by some physicians of eminence, especially in Germany, I deem a perfectly untenable doctrine, which does not merit a serious discussion; that is, if the words mind or soul are employed as synonymous terms, and are meant to express the immaterial agent or influence, which directs our reason and moral feelings; but if the same metaphysical physicians mean by the word mind, something cerebral or physical, something, in fine, belonging to animal organisation, there is no need for any further discussion, as the dispute would then be solely about words.

The brain is the only viscus of the human body which performs two functions that are essentially distinct from each other. There is, in fact, not a shadow of resemblance between them; and this justifies the conclusion that there are two distinct agencies operating on the

brain, to which the two classes of phenomena are to be referred. One of these agents is the living principle which is common to all the organs of the body, by the laws of which, its animal functions are performed and maintained; such as its nutrition or renovation, and the forming a peculiar stimulant, which is conveyed by the nerves to every part to which they are distributed. The other is that mysterious and spiritual agent, to which conscience, and reason, and indeed all the intellectual faculties, must be referred.

The living principle of the brain (considering this vital principle as a specific force) is at all moments opposed by the chemical affinities of its constituent elements. This is a general law to which all organised living bodies are subject, and it is on this account that the brain, like every other part of the body, is subject to many physical effects or diseases; but when such diseases occur, no one asserts that it is the living principle which is diseased. We do not dream of attributing physical changes to an immaterial force, and yet, strange to say, the soul, which is equally immaterial with the living principle, and is a force of a higher order, is still supposed by some to be subject to physical affections.

The symptoms which precede, as well as those which accompany, a paroxysm of maniacal insanity, have a greater analogy to those of inflammatory irritation than to any other known form of malady. We have no names for the numberless affections to which we give the names either of inflammation or of irritation, and as we cannot examine the brain of insane subjects until after their death, we cannot obtain very correct notions concerning them. Yet much has been done in this matter, chiefly by French physicians of great eminence, to whose discoveries I shall soon allude.

After an individual has suffered repeatedly from

paroxysms of acute mania, and especially when the powers of life begin to decline, or when the delirium has been long protracted, (say more than a year,) the alienation of reason is apt to become permanent, but the violent excitement ceases; the disease assuming a passive and atonic character, constituting the *dementia* of modern authors.

The excellent anatomical observations of MM. Foville and Delaye, are of infinite importance in throwing light on the true nature of mania. In fact, I am of opinion, that the brain of insane people has never been satisfactorily examined but by them; all previous dissections of that organ detected only the grosser kinds of organic lesion, such as tumors, ossifications, indurations, softenings, extravasations, and ulcerations, &c., all of which are the effects of chronic inflammation.

The attention of M. Foville and his friend, but more especially of M. Foville himself, was directed, 1st, to the morbid appearances of the cineritious substance, or the cortical part of the brain; 2ndly, to the alterations of the white or fibrous parts of the same; 3rdly, to those of the nerves of sense; 4thly, to those of the membranes; 5thly, to the bones of the skull and integuments; and, lastly, to the various appearances which are found in the heads of idiots.

It is to the first of these alone that I beg to call the attention of the reader, for it is these which throw additional light on the nature of mania.

So scrupulous were these physicians and anatomists, that distrusting, as every one ought to do, the fidelity of memory, they were careful to have a sound brain beside them, whenever they examined that of an individual who had died insane. And, therefore, I think that great confidence is to be placed in their observations. I am not aware that M. Foville has written any separate work or mono-

graph on alienation of mind, but in the *Dictionnaire de Médecine et de Chirugie Pratiques,* under the title of *Alienation Mentale,* will be found recorded his opinions, and the results of the anatomical researches of himself and his friend.

In acute cases of insanity, the cineritious part of the brain was discovered by Foville to be preternaturally red and congested. He says, expressly, that in acute cases, as soon as the membranes which cover the convolutions of the brain were removed, the cortical or grey part was discovered to be of an intensely red colour, resembling erysipelas, and that it was more remarkable in the centre of its substance than on its surface. He confesses that in some cases the grey substance does not appear, on first inspection, to be of a preternaturally red colour, but he asserts that even in such cases, if it be dexterously pared, and very thin slices of it cut off, so as not to lay bare the white part of the brain, a preternatural redness, varying from that of blood-red to a lilac colour, will generally be seen. Owing to the different degrees of inflammation or congestion with which this part of the brain is affected in acute mania, it puts on a marbled appearance, in which patches of a still deeper red hue and of preternatural softness are detected. M. Foville adds another important observation. He says that in acute cases he never found the membranes adhering to the cortical part, whereas in chronic cases the membranes were commonly found to be adhering, and the cineritious part to be more or less indurated.

The cineritious part of the brain, owing to its great vascularity, which may be detected by a good microscope, is much disposed to inflammation, and when we reflect that the rest of its substance is composed of the fine expansion of the extremities of the ascending fibres of the brain, and that by means of the commissural

fibres, it is connected with every part of that organ, we can easily comprehend how much it is exposed to morbid affections, both from common physical causes, and from passionate emotions of mind, and, in fact, by everything which may disturb the circulation of the blood or occasion extraordinary excitement in the nervous system.

Dr. Carpenter, in his Human Physiology, which has been only recently published, and of which work I procured a copy as I was preparing this part of the present commentary for the press, has embodied many valuable anatomical facts, which have led him to conclusions very nearly allied to those of M. Foville. I hope he will forgive me for copying a few of the passages which relate to the subject.

'Three principal sets of fibres may be distinguished in 'the white or medullary substance, of which the great 'mass (of the brain) is composed. These are the ascend-'ing fibres, which proceed from the sensory tract, and 'diverge from the thalami optici to the periphery; the 'descending fibres which converge from the periphery 'towards the corpora striata, and then pass downwards 'into the motor tract; and the commissural fibres which 'establish the connexion between the various parts of 'the periphery and the substance of the brain. It is on 'the very large proportion which the commissural fibres 'bear to the rest, that the bulk of the brain of man 'and of the higher animals chiefly depends; and it is 'easy to conceive that this condition has an important 'relation with mental operations whatever be our view 'of the functions of different parts of the brain. The 'different relative distribution of the grey and white 'matter of the cerebrum, from that which is elsewhere 'presented to us in ganglionic masses, naturally suggests 'the inquiry, how far this corresponds with what has

'been stated of their probable functions. It may be 'remarked, in the first place, that we have no evidence 'whatever, that the endowments of the fibres are in any 'degree changed by passing from the nervous trunks into 'the brain; and that it is only where they terminate in 'the grey substance, that such an alteration takes place 'in their structure, as to warrant an assumption, in the 'absence of other evidence, that their function also is 'altered.'—p. 215.

The great vascularity of the grey substance, as compared with that of the white, is an important circumstance in favour of the doctrine, that it is the seat of all great cerebral affections and changes, and that the white fibrous part, like the trunks of nerves, serves only to transmit impressions.

This hypothesis receives support from pathological anatomy. 'It has been frequently remarked,' says Dr. Carpenter, 'that, if we compare these cases of cerebral 'disease in which there is delirium, with those in which 'it does not occur, we shall find that it is most common 'in cases in which there is an inflammatory affection of 'the surface, or of the membranes extending from them 'into it; whilst in deep-seated inflammation, the most 'important symptoms are those which result from sym-'pathetic affections of the muscular system.' . . . 'All 'the cases, therefore, which have been referred to, in 'support of this diagnosis, tend to establish the proposi-'tion, that the superficial portion of the cerebrum is the 'part really affected.'

Inflammation of the membranes of the brain, whether acute or passive, may exist independently of any great affection of the cerebrum itself, although, as Lallemand observes, it is scarcely possible to conceive inflammation of the arachnoid membrane, which does not extend more or less to the grey substance with which it is in contact,

and, consequently, delirium is a necessary attendant on this affection of the arachnoid; but it does not follow that inflammation of the dura mater, or even of the pia mater, should produce delirium, because the inflammation may be confined to these membranes; in which case it is absurd to suppose that the mental operations can be effected.

In the commencement of this Commentary, I stated as my opinion, that when insanity was accompanied by palsy or epilepsy, there was a distinct cause for the insanity, and another for the palsy or epilepsy; in short, that they were not necessarily connected like cause and effect. The mere fact that each of these diseases may exist independently of the other, for many years consecutively, proves their want of necessary relationship, like cause and effect.

The organic lesions which have been detected in the heads of paralytic, epileptic, and cataleptic patients, are found, chiefly, in the white, medullary, or fibrous part of the brain, or else they are bony excrescences of the internal table of the skull, or tumors and ossifications of membranes which act by mechanical pressure; but I have not read of a case where the seat of these diseases was confined to the cineritious substance. Calmeil, indeed, assumes it to be the case, but he does not prove this by unquestionable dissections; whereas, Foville and his associates inform us that they have made several hundred dissections of bodies, in which there were well-marked alterations of the cortical part of the brain, which did not produce, during life, any other disease than in the intellects. Similar facts and opinions are mentioned by Bouillard.

Reflections on the Remedial Treatment of Mania.

That acute mania is nearly allied to true acute or active inflammation, is an opinion which has been pronounced, maintained, and acted on, by some physicians of great name and eminence; by none more so, than by the celebrated Dr. Rush of Philadelphia, and yet, I am convinced that both the doctrine, and the practice he recommends in conformity with it, are equally faulty and dangerous.

Having never had a lunatic hospital or private asylum for the insane under my care, I confess that my experience of mental derangement has been very limited as compared with that of those who have had such advantages; but as it was known to many that I had turned my attention to the study of alienation of mind at a very early part of my medical career, it may easily be comprehended that I have often been consulted in such cases, and that, in the course of a long life of general practice, I must have met with many cases of the kind. The result of what I have seen, and the reflections which have occurred to me, I am very willing to communicate, and to subject to the criticism of others. I shall confine my observations entirely to the effects of remedies and diet, for as to questions concerning the personal restraint of the insane, and the proper exercises and occupations for their propensities and intellects, when reason begins again to dawn in them, these subjects require more thought than I feel I can bestow upon it at my advanced age.

The first unsettled point regarding the remedial treatment of a paroxysm of acute mania, is, as to the propriety of blood-letting. The greatest authorities in favour of it are Drs. Rush, Joseph Frank, and Haslam. From a patient, who laboured under acute mania, and who at the time was in his sixty-eighth year, Dr. Rush took

away two hundred ounces of blood in less than two months: and from another patient four hundred and seventy ounces of blood in the space of ten months, by means of forty-seven venæsections. This was certainly not a very quick cure; but Dr. J. Frank relates the case of a young maniacal girl who was almost suddenly restored to reason on losing four pounds of blood at once!

It appears to me that Rush founded his practice more on theoretical conclusions, than on extensive and often-repeated experience. He says the reasons for copious blood-letting in mania, are, 1st, the force and frequency of the pulse, the want of sleep, and the general agitation of the patient; 2ndly, because insane patients, being seldom deprived of appetite, and having often a keener one than in their usual state of health, they must be subject to sanguineous plethora; 3rdly, the importance of the affected organ—the delicate structure of the brain, which makes it incapable of supporting for a long time, without being disorganised, any great morbid derangement, the risk of which is much increased by the incessant cries, vociferation, singing, and violent bodily movements of the maniac; 4thly, the want of an outlet from the brain, by which serous and other effusions from its inflamed vessels can be discharged; 5thly, the accidental cures which have resulted from the loss of large quantities of blood; 6thly, blood-letting, he says, is indicated, in consequence of the extraordinary success which has attended the practice in the United States, and especially in the hospital for the insane of Pennsylvania.

The two last reasons are those only which have an immediate reference to experience, and on these I am desirous of adding a few words, before I bring forward other witnesses either for or against blood-letting in

mania, and before I deliver my own opinions on the subject.

It must be well known to experienced physicians, that one of the very first overt acts of insanity with a certain class of patients is an attempt at suicide. It is to such cases that Dr. Rush confessedly refers in his fifth argument; and he justly observes, that several of those who try to destroy themselves by cutting their throats, but who do not divide the carotids, and consequently, who do not die from the wound, recover their reason almost immediately, in consequence of the loss of blood. That the fact is correctly stated, I can affirm from experience, a few similar cases having fallen under my observation. In two of these cases the patients were the descendants of insane parents. But the insanity which impels a patient to destroy himself, is not acute mania. The brain and the intellects of the patient are in a totally different condition. It is true, that a raving maniac may destroy himself; he may leap out of an open window, dash his head against a wall, or run into a river, but he is a creature entirely without consciousness of what he is about, like a person in the delirium of a fever, who is capable of commiting similar acts, if not restrained. Whereas the insane person who meditates self-destruction, and is compelled to it by his malady, has always enough of reason and consciousness remaining to direct his actions. He conceals his intentions with cunning, he watches his opportunity, he has various contrivancies whereby to attain his object; in one word, he is a monomaniac, whose brain is only partially diseased, and which disorder in its commencement is frequently associated with, if not caused by, partial venous congestion.

Suicidal monomania is an affection of the brain which, whether it be symptomatic of hepatic congestion, which

it often is, or purely cerebral, is more allied to the melancholia of ancient authors than to acute mania; and whatever difficulty there may appear to be in explaining the fact, experience teaches us that in this particular variety of insanity, the loss of a considerable quantity of blood is often (not always) beneficial. No good argument in favour of copious blood-letting in acute mania, therefore, can be founded on such cases as these.

The sixth reason of Dr. Rush is more to the point; but then, its force rests solely on the assertion that in the Lunatic Hospital of Pennsylvania, acute mania is successfully treated by copious blood-letting. Admitting the assertion to be well-founded, I would observe that a difference of climate may account for the result. The heat of summer in that part of the world is much greater than in Great Britain, and in hot climates, I believe, blood-letting in acute mania may be often necessary. This celebrated American physician advises the withdrawing a very large quantity of blood, viz. from twenty to forty ounces at once, on the first attack of a paroxysm, provided the patient is not seized with syncope. The effects of copious blood-letting in the commencement of insanity, he says, are quite extraordinary; for according to him, it renders all other remedies useless, inasmuch, as it cures the disease in a few hours! This is a bold assertion, and is also very hazardous practice, as I hope to be able to prove.

Dr. Rush, however, is supported in his opinions by Drs. Haslam and Broussais, and, to a certain extent, by M. Foville. The first of these authors says, when the patient is strong and of a plethoric constitution, and when the disease is recent, blood-letting is highly advantageous. If Dr. Haslam had stopped here, his advice would have been a safe one, but when he adds, that his experience justifies him in saying that it is in general the most

powerful remedy which can be employed in the disease, he assumes that to be true which is positively denied by men of equal experience and talents with himself. The quantity of blood, however, which Haslam recommends to be withdrawn is not extravagantly large; the average quantity being from eight to sixteen ounces; which he advises to be repeated if necessary.

Broussais, whose vanity led him to assert that his system of *médecine physiologique* had first opened the understanding of physicians to the true nature and treatment of insanity, prefers the taking away large quantities of blood by leeches to venæsection.

M. Foville is more guarded and prudent in his recommendation of blood-letting than the authors just mentioned. He is evidently influenced by his anatomical discoveries, which lead him to conclude that the disease is truly inflammatory.

Let us now attend to the evidence of two witnesses, who for long and extensive experience in the treatment of mental derangement, and for fidelity in their narrations, have not as yet been surpassed,—I mean Pinel and Esquirol.

It was an invariable practice with us, says Pinel, on the admission of the insane into the hospital, to interrogate the relatives of the patients if they had been blooded, and also what had been the effects; and the almost constant answer to such questions, in the cases in which it had been employed, was, that the alienation of mind had been aggravated by it.

Two young people, he continues to say, of the same age, and of a similar constitution and temperament, were admitted on the same day. One of them, who had not been blooded, was cured in two months. The other, who had been copiously blooded, fell into a state of idiocy. She did not recover her speech until the end of

the fifth month after her admission, and she was not restored to reason until the end of the ninth month. He mentions, among many other cases of a similar kind, one of an unmarried woman, thirty-six years of age, who was attacked with acute mania, whose catamenia had been suppressed, whose vociferations were loud and incessant, whose face was congested with blood, and her eyes glistening, and who, in consequence of such symptoms, had been blooded at the ankle. The quantity of blood which was withdrawn was not large, and yet she fell into a state of idiocy, from which she did not recover until two years afterwards.

Pinel says that his appointment to the hospital was the beginning of an epoch during which blood-letting was abolished. Such exclusive and general rules for the practice of an experimental and imperfect art cannot be praised; but knowing Pinel personally, as I had the honour to do, I am perfectly persuaded that his dread of blood-letting in mania resulted from experience alone, and not from any hypothetical prejudice. His opinion was the sincere conviction of his mind.

M. Esquirol, who, as the successor of Pinel, had equal opportunities of seeing a vast number of insane, writes with much greater reserve concerning the danger of blood-letting in mania. He disallows its pretended utility. He says, that he has seen the disease increase in violence after one or more blood-lettings; and he remarks that a profuse flow of catamenia is often followed by the same bad consequences, events which it is difficult to account for if we are to believe that either cerebral inflammation or sanguineous plethora are the causes of the complaint. Blood-letting in mania is condemned by Georget, who, both while he was assistant at the Saltpétrière and afterwards, had extensive experience in the treatment of the insane.

In some cases of acute mania in young, vigorous, and plethoric patients, I have ordered from ten to fourteen ounces of blood to be taken from the temporal artery at once, and in other cases I have ordered several leeches to be applied to the neck. But I must admit that I never saw it produce any speedy beneficial effect; neither have I seen it followed immediately by idiocy. At first I flattered myself that I could diminish the violence of the paroxysm, and calm the turbulence of the patient by means of this remedy; but in general I was disappointed in the expectations I had formed, and ultimately I never ordered blood-letting but in cases where I dreaded the rupture of a blood-vessel in the head from the violent agitation and vociferations of the patient.

I will not swell this Commentary by quoting the opinion of authors as to the effects of other remedies which are less hazardous in their application than blood-letting, but I shall at once state the result of my limited experience concerning them.

I must, however, first mention the opinion I formed of the nature of the disease long before I read M. Foville's account of his dissections, for, unquestionably, the conclusions I came to, after studying the disease as carefully as I could do, without the light which M. Foville has thrown on it, had great influence on my practice.

I differ from M. Foville in the conclusions he draws from his discoveries. He considers the very vascular appearance and congestive state of the cineritious part of the brain as a proof of true inflammation, and whoever reads his remarks on blood-letting, as opposed to the experience of Pinel, must see, I think, that he is influenced by this view of the disease. He asks if the anatomical character of the brain in acute mania and the adhesions which are so commonly detected in chronic

cases, are not evident proofs of inflammation? and if so, are we not authorised, he adds, to employ the most active antiphlogistic remedies to subdue it in its commencement; for, he continues, what hope is there that so delicate an organ as the brain can perform its important functions after adherence and other organic lesions are permitted to be formed? This would be reasonable if we really possessed any antiphlogistic remedy on which we could rely for arresting the cerebral affection which is peculiar to insanity. I have always been of opinion that acute mania was more nearly allied to inflammation than to any other disease; but I could not banish from my mind the certitude, arising from experience, that there are a variety of congestive states of blood-vessels to which we give the appellation of inflammation, all of which, without one exception, are perfectly distinct from active inflammation, and which moreover differ entirely from each other; in short, that they are specifically distinct diseases, though they all exhibit preternatural congestion, vascularity, and other signs that are common to inflammation in general; such are, for instance, cancerous, gouty, syphilitic, variolous, vaccine, scrofulous, and herpetic inflammations. How few of these can be alleviated by copious blood-letting! In how many of them is it not a dangerous remedy! How various is their treatment!

Until some lucky and unforeseen accident discovers to us a specific remedy for curing maniacal inflammation, I am of opinion that the safest practice consists in the judicious employment of a palliative or symptomatic mode of cure.

The paroxysm generally runs a course of several weeks' and frequently of several months' duration. It passes from an acute to a passive state, and the collapse is commonly proportionate to the previous excitement. In

general the transition is gradual, but now and then it is sudden. In this atonic state the intellects of the patient still continue to be confused, but the expressions of passionate emotion are calmer. In a few rare and happy cases the patient is suddenly restored to reason after a few weeks of delirium.

Of all the remedies which I have hitherto tried in the hope of subduing the excitement of the patient in the acute stage of the paroxysm, there is none which has rendered me more service than the tartrate of antimony in doses of from one-fourth to one-sixth of a grain every four hours. At first it does not seem to do much good. It must be employed uninterruptedly for some weeks. If any one doubts its efficacy, from not being able to discern a marked amelioration of the patient, he need only lay it aside altogether, and then mark the consequences. I have often combined it with camphor in moderate doses, but the camphor does no good in the beginning of the paroxysm. It is more useful afterwards.

At the same time that I administer the tartrate of antimony, I cause the patient to be put daily into a tepid bath, allowing him to sit in it, if it can be so managed, without greatly coercing or irritating him; care being taken, however, not to expose his neck and chest to the cold air for fear of bringing on catarrh. He ought to remain from half an hour to forty minutes in the bath.

In the first week of acute mania, it is almost impossible to employ this excellent remedy with advantage, owing to the great difficulty of commanding the patient; but by means of a proper dress it may be managed.

If the tartrate of antimony does not keep the bowels of the patient sufficiently open, a sufficient quantity of any purgative salt may be given so as to operate once or twice every day.

In the later years of my practice I gave a grain of the

extract of colchicum, combined with four grains of the compound extract of colocynth, and a quarter of a grain of the extract of belladonna at bed-time; and I thought I derived advantage from this kind of sedative and aperient combined.

The patient's head ought to be shaved in the commencement of mania, and it ought to be frequently sponged with cold vinegar and water. When the scalp is very hot and the delirium excessive, it is useful to apply six or eight leeches to any part of the head.

Many recommend the application of a blister over the whole head, expecting benefit from it as a counter-irritant. It is, however, a very uncertain and doubtful remedy. In some cases it appeared to me to determine the blood to the head, and to augment the violence and irritation of the patient. In no case did I ever see it calm the patient, except in passive or chronic mania.

The pouring buckets full of cold water from a height on the head of maniacs when furious, is strongly advised by some. I deem this also a very doubtful, if not a hazardous remedy. In general anything which gives a sudden and violent shock to the brains of a lunatic is bad. Electricity is pernicious in the highest degree.

In the acute stage of the delirium the patient ought to be restricted to low diet. He should not be allowed animal food. Whey or milk and water, with bread, or water-gruel and bread, are sufficient.

But when the collapse or atonic state commences, the whole treatment must be changed, though, at first, with great prudence and as if feeling one's way.

When the face becomes pale, the pulse feeble, and the agitations of the patient are of a passive though restless kind, whatever the state of his intellects may be, he will be found to require support both from diet and medicines. In such a state, I have often given the pa-

tient wine, valerian, and even cinchona, with great advantage.

I have mentioned colchicum as a useful medicine in maniacal delirium. I do not limit its employment to acute mania. I have derived benefit from it in various forms of insanity in the first or second attacks of the disease.

He who hopes to cure insanity of any kind after it has become chronic, or after many attacks of it, even when only periodical, must have more faith in medicine than I have. The dissections of Foville and others explain the reasons.

On Crimes which are imputed to Insanity.

Cases of homicide, arising from sudden provocation, by individuals of very quick feelings and irascible tempers, have at times come before the criminal courts of most countries. In some of these cases it has been pleaded, in defence of the accused person, that he laboured under temporary insanity at the time he committed the act, and consequently, that he was an irresponsible agent, who ought not to be condemned, but treated like a lunatic.

This opinion is at present maintained by many learned men, who argue first, that the crime is not premeditated, and, secondly, that the disordered state of the intellects, which is caused by sudden and violent emotion, is identical with the delirium of a maniac, and therefore, that, as reason is destroyed, there can be no freedom of the will.

A more important subject of inquiry than this cannot well be imagined, whether it regards the human mind in its aberrations or the impartial and rigid administration

of justice; and its importance becomes more serious when we witness the extent to which the hypothetical proposition is at present carried by men who are celebrated for metaphysical science.

I have before me a learned dissertation of Professor Grohmann of Hamburgh, which bears the following title in German: *On the Diseased Affections of the Will, which determine the Irresponsibility of Criminal Actions;* and another by M. Worbe, licentiate of jurisprudence, and physician at Dreux, in the department of Eure and Loire, on the following question, *Whether a woman with child, who is accused of theft, may plead in palliation of the offence the longings of pregnancy.* M. Worbe thinks the justification sufficient; but as the judges in the case alluded to were of a different opinion, and condemned the woman to confinement in prison for a few days, he laments the disregard of medical opinions, and concludes with the adage *summum jus, summa injuria.*

In this country a physician of eminence who has made the human mind an object of serious study, arrives at the conclusion that there is a variety of true insanity without either delirium or hallucinations, to which he gives the name of *moral insanity.*

To this disease, Dr. Pritchard (the inventor of the name) thinks that the actions of many vicious as well as eccentric characters are to be ascribed. He says the disease consists 'in a morbid perversion of the feelings, affections, and active powers, without any illusion of erroneous conviction imposed upon the understanding.'

Under the same denomination it appears to me that Dr. Pritchard comprehends a number of diseased affections, as well as varieties of moral character, which have no similitude to each other or even to true insanity.

The bad consequences of such a generalisation as this, especially on the minds of medical men who may be

called on as witnesses in litigated questions, will, I hope, justify the remarks I have to make on the great difference between the perturbation of mind of an impassioned criminal and that of a madman.

To put the question in a clear point of view for discussion, I shall extract from Dr. Pritchard's work, his first case of moral insanity. It is cited by the justly celebrated Pinel, but the translation is that of Dr. Pritchard, which I give in his own words. The case is a sad, but an interesting one, which may serve as a warning to many.

'An only son of a weak and indulgent mother gave 'himself up habitually to the gratification of every caprice 'and passion of which an untutored and violent temper 'was susceptible. The impetuosity of his disposition 'increased with his years. The money with which he 'was lavishly supplied, removed every obstacle to the in-'dulgence of his wild desires. Every instance of oppo-'sition or resistance roused him to acts of fury. He 'assaulted his adversary with the audacity of a savage; 'sought to reign by force, and was perpetually embroiled 'in disputes and quarrels. If a dog, a horse, or any 'other animal offended him, he instantly put it to death. 'If he went to a *fête* or any other public meeting, he was 'sure to excite such tumults and quarrels as terminated 'in pugilistic rencontres, and he generally left the scene 'with a bloody nose. This wayward youth, however, 'when unmoved by passion, possessed a perfectly sound 'judgment. When he became of age, he succeeded to 'the possession of an extensive domain. He proved 'himself fully competent to the management of his 'estate, as well as the discharge of his relative duties, and 'he always distinguished himself by acts of beneficence 'and compassion. Wounds, law-suits, and pecuniary 'compensations, were generally the consequences of his

'unhappy propensity to quarrel. But an act of notoriety 'put an end to his career of violence. Enraged with a 'woman who had used offensive language to him, he 'threw her into a well. Prosecution was commenced against 'him; and on the deposition of a great many witnesses, 'who gave evidence of his furious deportment, he was 'condemned to perpetual confinement in the Bicêtre.'

In this case the life of the murderer was spared, and he was punished like a lunatic who commits homicide.

It would not become me to make any other remark on the sentence of the French judges in this case, than that it was truly humane and compassionate. Perhaps, the testimony of so celebrated a physician as Pinel had some influence on their minds in coming to the decision that the young culprit was an irresponsible agent. His opinion was that the unhappy youth laboured under *un emportement maniaque sans delire*, or in English, maniacal rage without delirium.

This is both a guarded and an ingenious description of the young man's mind, but it leaves us in doubt whether Pinel employed the adjective maniacal in any other sense than as a superlative, to denote the excess of his fury. It is by no means clear that he meant it to denote a state of disease. But Dr. Pritchard pronounces the case to be one of true insanity which has not as yet sufficiently attracted the attention of medical men.

I would remark in the first place, that there is not a fact or circumstance connected with the history of the life of the young culprit which proves that he laboured at any time under true insanity, or even under the cerebral excitement which so frequently precedes for a time an attack of maniacal delirium.

It is impossible to read the history of this unhappy youth's short career, as narrated by Pinel, without feelings of commiseration for his fate. He describes him as

having been possessed of a naturally good understanding, and a generous disposition, except when under the influence of short but violent fits of passion. His affections and moral feelings were good, and at no times were they perverted, as we know them to be in every variety of insanity; but it seems that his violent temper had never been subdued or disciplined in his early years. He had not been taught to dread either immediate or future punishment for any of his errors or vicious conduct. On the contrary, his fond but weak mother supplied him with the means of gratifying every wish, until at last his desires and their attendant passions became too strong for a neglected understanding. But we are told that when his paroxysm of anger subsided, he was desirous of making the best amends he could for the injury he had committed, by many acts of kindness and generosity. Where is the proof that the moral feelings and sentiments of this youth were in a diseased state?

We cannot know what the physical condition of the brain is, when under the action of passionate desires, and consequently we cannot compare it with what we now know of the state of that organ in mania, (see preceding Commentary,) but the manifestations of mind and feeling in the two states, are so perfectly distinct in one essential character, that it only requires to be pointed out to be acknowledged.

The great psychological distinction between passionate emotion of mind in a sane person and the mental affection of a maniac, or the hallucination of a monomaniac, consists in this, that the former ceases instantly and entirely when the object of the passion is accomplished, or, in other words, when the desire is gratified; whereas insanity is not arrested by the commission of a crime. The murderous disposition of a diseased brain cannot be subdued but by restraint or by cure. A madman would assassinate

several people in succession without distinction, consciousness, or remorse, if not prevented by force.

It is of no importance from what desire a turbulent or vehement passion arises. It may be from revenge, lust, avarice, or any other vile and criminal impulse. The moment the desire is gratified, the passionate emotion ceases in a sane mind, and, according to circumstances, the criminal may become calm and collected, or he may be seized with opposite emotions and feelings, such as those of remorse, fear, and shame.

It cannot be maintained by any solid argument, or even shadow of truth, that a man who is sane in the ordinary sense of the word, though he may be agitated by the most violent passion which can be imagined, is wholly deprived of his reason like a lunatic. However suddenly roused and excited he may be by hatred, lust, or avarice, &c., he has always a definite object in view, which he chooses or determines to attain.

In our fallen state, evil propensities and bad passions are the inheritance of every descendant of Eve, but experience teaches us that there is not a propensity or passion to which we are subject, which may not be weakened, and rendered even inoperative, by accustoming an individual to associate with its first impulse or suggestions, emotions of a contrary nature. Hope and fear are the great engines. It is on this principle that moral education depends. It is in this way alone that vicious propensities in youth can be corrected. But it is too much to say that because many unfortunate individuals have never been accustomed by due instruction and discipline to exercise their reason and to associate in their minds the dread of future or temporal punishment with evil propensities and sinful acts, therefore they ought to be considered as irresponsible agents and lunatics.

That they have been so, however, appears to be esta-

blished by the preceding case, to which some analogous cases and sentences might be selected from the records of criminal courts in this country.

On the other hand, a great number of instances of true monomaniacs whose lives ought to have been spared have perished on the scaffold, like criminals who were responsible for their actions.

Such transactions have happened within a very few years past, not only in this country, but also in others where the magistrates are enlightened, and the laws on the whole humane, and therefore nothing can prove more clearly either the defects of medical evidence or the incorrect conclusions which men of sound judgment may draw from the nature and acts of monomaniacs.

It is argued by some that the example of punishment in these cases is equally beneficial to society, because it will deter others from similar crimes. Deter whom? other monomaniacs? We might as reasonably expect that the example of punishment should prevent inflammation of the stomach.

Monomania arises from a specific disease of the brain, as much as acute mania does; but as it is confined to a part of the organ, hallucinations are engendered by the sensations of that diseased part. These impose on the judgment of the individual. To imagine that the sensations of a diseased portion of brain are to be prevented or cured by hope or fear, is, to say the least of it, a singular prejudice arising from ignorance of the disorder. If the law of the land ordained the condemnation to death of every monomaniac who committed homicide, it could not by possibility prevent the breaking out of the disease in others who are disposed to it.

But if, in consequence of such an admission, it is to be argued that the state of brain of a vicious and irascible individual when suddenly roused to passion, is also

diseased, and resembles that of a maniac or monomaniac, and consequently that his life ought not to be forfeited when he commits murder, such a conclusion is perfectly unreasonable, as it is founded on a supposed analogy where there is no similitude.

Cases in point will probably illustrate the distinction much better than all the attempts I have hitherto made to make it intelligible. We shall select two that are common, one of a sane, the other of an insane murderer, in both of whom there was the same passion, namely, the desire of relief from the apprehension of utter destitution.

The unfortunate father of a family of young children, a man in the prime of life, of acute feelings and passionate temper, almost wholly uneducated and irreligious, is driven to despair from want of employment and extreme poverty. In his distraction how to procure food for himself and family, he breaks into a house at night, to rob it, but is suddenly and unexpectedly surprised. In the confusion of his thoughts he stabs the person who is the obstacle to his wishes, adding murder to the act of burglary. What then happens? Does his derangement of mind continue? No: his reason is not destroyed. He attempts to escape, or being overcome by fear, want, and remorse, he may deliver himself up to justice.

Let us now turn to a monomaniac mother, whose hallucination consists in the notion that she and her children are doomed to perish from want, though, at the time, she is possessed of fortune, and of every comfort it can procure. To free the children from a lingering but sure death is her first thought. In her despair she poisons them; but at the time she procures the poison she also buys toys for their amusement. She caresses them, and mixes the poison with their food. Does the disorder of

her brain cease when she sees them lying extended and dead? Does she attempt to fly? Does she show any grief or remorse? No; she invites the neighbours to see how lovely they are, and how sweetly they sleep; and shortly after, when an opportunity offers, she commits suicide, the dread of impending destitution still haunting her imagination and producing the feeling of despair from which she seeks relief.

To assert that there is a strict similitude between the mental condition of these two individuals, and that they are equally irresponsible agents, from the influence of similar causes, which disturb their reason, is a conclusion which the common sense of mankind at once rejects: but which a number of learned physicians and metaphysicians of the present day affirm: and in fact they have extended their doctrine to the palliation of almost every vicious and criminal act.

We have no means of examining the brain of individuals when they are agitated by passionate emotions, consequently we cannot compare its appearance with that which is now known to belong to maniacal disease; but if we lay aside metaphysical reasoning and prejudices, and act the part of attentive observers, we may surely discover that the passionate emotions of a sane and of an insane individual are very different; and it is probably owing to the want of due attention to this distinction that so much room has been given to metaphysical hypothesis.

Passionate emotion of mind in a sane person, however violent it may be, is of a fleeting and unsteady character, varying with circumstances, which plainly demonstrate that the person is not wholly deprived of understanding; whereas, in the insane, the disturbance of reason is protracted, and, however varied his relative position may be,

either as to persons, place, or time, if he be not restrained by force, there is no variety in his language or conduct.

Will any one pretend to say that if the young French murderer whose case is described by Pinel as one of *emportement maniaque sans délire*, and which Dr. Pritchard calls moral insanity, had been opposed by a resolute person, who dared him, at his peril, to assault the defenceless woman, that he would have proceeded in his attempt to throw her into a well? and as to the second case already mentioned, I ask if the housebreaker had been surprised by a well-armed and bold person, instead of a timid and unarmed one, what would have been his conduct? He would either have attempted to escape, or after casting a quick glance at the strength and weapon of his antagonist, he might have assaulted him in the hope of killing him: or finally, he might have fallen on his knees and implored mercy. Such a variety of motives from which he chooses one according to circumstances, show evidently that his conduct is guided by understanding, though at the time he is under violent excitement; and yet it is pretended that such a man is to be considered irresponsible for his crimes.

I do not feel myself called on to take part in the fruitless discussion concerning the slavery (*unfreiheit*) of the human will. The doctrine of necessity has occupied the thoughts and pens of men of the first talents in almost every school of Europe, and has left the question as much undecided at present, as it was when first proposed. Such, I am bold enough to say, it will continue to be to the end of time, for the data on which arguments are built concerning it, are referred to personal experience, consequently to a tribunal where unanimity of opinion is not to be expected.

If revealed truth, which declares that we are to answer

for the deeds we commit in the flesh, is discredited, it is not by metaphysical arguments that we can hope to settle our faith; but this, every one must needs know with certainty, that in this country, and in every other civilised state which is careful of the interests of society, the laws are founded on the doctrine of the freedom of the will, as it is called, or on human responsibility, and also on the belief that examples of punishment and the dread of future condemnation will influence the determinations and actions of men until they are bereft of reason by real disease. To the judge must be left the task of considering the circumstances which palliate offences and make criminals objects of special commiseration and mercy, but the attempt on the part of learned doctors in law and medicine, to confound vice with insanity, and consequently to condemn the right of human punishments, I consider as one of the many dangerous innovations which the proud philosophy of the nineteenth century has produced.

EXTRACTS

RELATIVE TO

THE TREATMENT OF SYPHILIS WITHOUT MERCURY,

MORE ESPECIALLY TO

THE METHOD WHICH HAS BEEN EMPLOYED DURING MANY YEARS, WITH PERFECT SUCCESS, IN THE GENERAL HOSPITAL OF HAMBURGH:

TO WHICH IS PREFIXED

A SHORT ACCOUNT OF OTHER TRIALS OF A SIMILAR NATURE, AND AN ATTEMPT TO EXPLAIN THE REASON WHY A RIGIDLY LOW DIET IS ESSENTIAL TO THE SUCCESS OF THE TREATMENT.

EXTRACTS,

&c.

In the Second Commentary, and at that part which relates to the action of contagion, I ventured to propose an hypothesis which seemed to me to explain the short duration of febrile diseases of a contagious character, and the unlimited one of infectious disorders, which are not accompanied by any specific form of fever.

That hypothesis was deduced from the facts and arguments employed by Dr. J. Liebig in support of his ingenious doctrine concerning the general mode of action of certain animal poisons which he thinks contaminate the circulating fluids before any symptoms of disease appear; a doctrine which agrees with almost all the known effects of contagious and many other animal poisons. His doctrine, however, left unexplained the curious fact that some animal poisons of an infectious kind, such as those of small-pox, measles, the plague, &c., produce diseases which are comparatively of short duration, whether they terminate in convalescence or in death; while others, such as syphilis and yaws, which are unaccompanied by fever, run an unlimited length; and yet I am convinced that if Dr. Liebig had given it the slightest consideration, he must have seen that the ex-

planation follows as a natural consequence from his theory, therefore, I assume no merit to myself in proposing an hypothesis on this subject.

If it be true, as Dr. Liebig seems to prove, that some animal poisons, on coming into contact with the living blood either by means of a wound or by imbibition, immediately occasion a series of chemical transformations of some of the elements of that fluid, agreeably to the laws of chemical metamorphoses, it follows, of course, that if such parts of the blood as are susceptible of these changes, be not supplied, the disease must terminate. In febrile diseases of an infectious character the chemical metamorphosis is put a stop to by the very nature of fever, which destroys both the desire for food and the process of chymifaction; and consequently, the supply of new elements for the further formation of new virus is cut off; but in syphilis and yaws, which do not affect the brain or vital functions for a long time, the patient, by daily taking food in abundance, supplies every day new elements for the production of fresh quanties of poison, and consequently, the disease goes on and is protracted indefinitely.

It is well known that in hot climates, where the perspiration is great, and the inhabitants are naturally of sober habits, living chiefly on vegetable diet, or, at all events, on little animal food, syphilis is a comparatively mild disease, the symptoms disappearing without the use of mercury.

During the occupation of Portugal by the English army our military physicians and surgeons had frequent opportunities of observing with what ease the natives were cured of the worst venereal symptoms, by means of their Lisbon diet drink, without mercury, while the English, who, in whatever country or climate they sojourn, seldom relinquish their carnivorous habits, were

cured with difficulty even of symptoms which in the commencement were slight.

Practical surgeons must, I think, have observed that syphilitic patients who habitually eat large quantities of food, are cured with more difficulty than those who eat sparingly; and, in fact, a mercurial course seldom corrects confirmed lues until it has previously reduced the strength and plethoric condition of an individual.

The trials which were made by Mr. Rose, upwards of forty years ago, in the hospitals of the Coldstream Regiment of Guards, to cure syphilis without mercury, are well known to most medical men. The remedies employed were occasional venæsection and an antiphlogistic regimen, together with the administration of sarsaparilla, mezereum, sassafras, or a decoction of the same. But although success attended many of the trials, this new method failed to produce anything like a general conviction that the disease could be perfectly and radically cured with certainty without mercurial medicines, and, in fact, without sustaining for a time what is called mercurial action.

But about the same time, or rather some years after, a celebrated physician and surgeon of Copenhagen, Professor Winslow, gained much celebrity by his success in curing inveterate cases of syphilitic affections, which had resisted the action of a mercurial course.

It appears from a narrative of his method and its results, which he sent to a medical friend at Stockholm, (Dr. V. Schulzer,) and which was read before the Royal College of Physicians of that city in 1811, that the diet and remedies he employed were as follows.

The diet which was adhered to was—

For dinner and also for supper:—from four to five ounces of very lean meat, either roasted or boiled; the meat to be either beef, mutton, veal, lamb, or roebuck.

The same weight of wheaten bread was also allowed at each of these meals. No other food or meal was permitted to be given during the twenty-four hours.

The medicaments which he prescribed were as follows :

℞. Extracti conii ʒvij.

Pulv. herbæ conii ʒj.

These were to be beaten into a mass and divided into pills of two grains weight each. Three of the pills were to be taken every forenoon, and the same number every evening.

At the same time the patient took, as common drink, a decoction of sarsaparilla, two pounds of which were made with two ounces of the root. This quantity was to be consumed in twenty-four hours, and no other kind of drink was permitted. But as a succedaneum for the sarsaparilla, which was deemed too expensive for the poor, a decoction of the radix chinæ of the same strength was substituted.

Mezereum and sassafras were added occasionally to these decoctions in affections of the periosteum.

Winslow asserts that the success of this mode of treatment was very great, but he makes use of an expression which is sufficient to convince any very experienced physician or surgeon that the diseases which yielded to these remedies were not purely syphilitic, but were of that mixed character which arises from syphilis and the abuse of mercury in cold climates, and which, for the last forty years of my life, I have always considered and treated as a variety of scrofula. He says expressly in his letter to Dr. Schulzer, that his greatest success was in those cases in which the continued use of mercury had not only failed to relieve, but even aggravated the symptoms. This is quite enough, in my opinion, to show what the true character of the disease was.

I now turn to the truly interesting cases of undoubted

and unmixed syphilis in almost every form and stage, which were cured without the administration of any preparation of quicksilver whatever, from the beginning to the termination of the disease, and in which the only remedies employed were low diet, chiefly of a vegetable kind, mild saline aperients, the tepid bath, and bodily rest.

The city of Hamburgh contained in its general hospital, at the time I visited it, now upwards of twelve years ago, about fourteen hundred patients.

It is an honourable testimony to the humanity and liberal spirit of that great commercial city, not only for the funds by which it is supported, but for the unlimited confidence which is placed in the professional gentlemen to whom the lives of so many individuals are entrusted.

The physicians and surgeons of the hospital are at liberty to order any diet or employ any medicines which their judgment approves: they are under no control but that of their conscience.

As there is no school attached to this institution, the physicians and surgeons have no motive for inventing dazzling theories, by which several of the German universities and academies have acquired temporary fame. Nevertheless the Hamburgh hospital has deservedly gained a truly European celebrity wherever the German language is understood, or where its *Annals* have been translated. Among its surgeons, Dr. J. C. G. Fricke is greatly distinguished, not only as an able and successful practitioner, but as an estimable and worthy man, on whose veracity the most perfect reliance may be placed.

This slight tribute of praise is not offered undesignedly, for the facts I am about to relate require that the moral character, as well as the talents of the inventor of a new method of treating well known diseases, should be known:

and it will not be denied that the cure of syphilis in all its stages and forms, by means of low diet and daily doses of epsom salts in moderate quantity, was, at the time alluded to, an innovation in the old mode of treatment, and even a new variety of other anti-mercurial methods of cure.

But there is another good reason for adverting to the talents and experience of Dr. Fricke, which is, that a very eminent surgeon of London, Mr. Herbert Mayo, in his valuable practical *Treatise on Syphilis*, implies that he (Dr. Fricke) has allowed his judgment to be imposed on.

After summing up the arguments for and against the mercurial method of treatment, he concludes by observing, that 'The alleged mildness of the secondary symp-'toms, when mercury has not been given, has most likely 'arisen from the cases with which they are contrasted 'having been cases where mercury has been administered 'more by routine than with discrimination, and, there-'fore, often in excess. It is in the same manner that I 'am persuaded that Dr. Fricke of Hamburgh, one of the 'ablest advocates of the non-mercurial treatment, has 'suffered himself to be deceived.'—p. 45.

The whole of the volume from which I have made the following extracts, is full of observations which are entirely practical. I lament that the work has never appeared in an English dress. It is the fourth chapter only which concerns us at present.

I have made very copious extracts from the Annals, and I feel confident that I need not make any apology to the practical surgeon of this country for having done so. The subdivisions and classifications of some of the commonest appearances of syphilis by Dr. Fricke and his assistant colleague Dr. Gunther, may indeed appear to some English surgeons to be unnecessarily

minute, but when we find that each of these required an appropriate treatment, the value of the whole as a body of practice will be admitted.

Dr. Fricke opens the chapter in the following manner, which is expressive both of his motives in publishing an account of his experiments, and the precaution he took against the chances of deception as to the results.

'I consider it my duty, in consequence of the extensive 'experience I have had in the treatment of syphilis, to 'comply with the request, not only of the medical profes- 'sion at large, but also with that of many of my col- 'leagues, of laying before them an impartial answer to the 'following question, *What are the varieties of syphilis, 'which are curable without the internal use of mercury, and 'what are those which resist it?*

'I may remark that arguments accompanied by satire, 'irony, and animosity, cannot produce conviction; that 'observation and experience alone can settle the inquiry. 'The observations, however, must be satisfactory, that is, 'the cases sufficiently clear and numerous to lead to a right 'conclusion.

'In order to avoid the reproach of want of due cau- 'tion, I at first selected out of the great number of 'patients, such cases only as were considered to be pecu- 'liarly fitted for a *non*-mercurial course of treatment. 'Contrary to my expectations, the result proved so satis- 'factory, that I deemed myself justified in subjecting 'every variety of the venereal disease to the same method 'of cure, for I thought I had taken every precaution 'against self-delusion. I imagined that a given number 'of cases might determine the question, and settle the 'point of dispute as to the forms of syphilis which could 'or could not be cured, without the administration of 'quicksilver.

'At present there are a thousand cases which have

'been treated without mercury—a number which, in 'regard to any other disease, would be thought sufficient 'to establish a rule of practice. Still I do not consider 'the trials as terminated, for it is not the number of cases 'alone, but the course of time, which must decide. I do 'not wish, however, to withhold the observations any longer 'from the profession at large, but to encourage medical 'men to repeat the experiments, and judge the matter 'with impartiality. By such means the truth will at last 'appear, although a century perhaps may elapse before 'victory decides in favour of one of the two methods. I 'must, however, declare that I am not of opinion, that mer-'cury is to be entirely banished from the catalogue of anti-'syphilitic remedies, or that venereal patients ought for 'the future never to have recourse to it. It will always 'remain a powerful medicine. But whether I am to be 'justified by all the circumstances which have occurred 'in not exhibiting mercury in any case of syphilis, the 'impartial perusal of this treatise must decide. I shall, 'therefore, continue to practise by the same method; and, 'as I hope, without incurring the reproach of partiality, 'which has hitherto been levelled against the advocates 'of the non-mercurial treatment of syphilis, and I shall 'from time to time lay the results before the public.

'At my request, Dr. Gunther, assistant surgeon to the 'general hospital, has undertaken to arrange and com-'municate the observations. It is he who makes the 'entries and conducts the journal of the syphilitic 'patients; an office which he has fulfilled with distin-'guished zeal and talent. In applying to him for this 'purpose, I hope to be considered as acting most impar-'tially, since Dr. Gunther has not the right of publishing 'anything which is not to be found in the day-book of 'the hospital. He has given me his promise not to in-'troduce any learned quotations, or to affect a rhetorical

'style, or to decorate the subject with any theoretical 'opinions, for such a subject as this requires only a dry 'detail of facts.'

The comparative trials, the results of which were published in 1828 by Dr. Gunther, had occupied four years, namely, from January 1824 to the end of 1827.

In 1830, I had an opportunity of visiting the hospital of Hamburgh daily, and the pleasure of becoming personally acquainted with Dr. Fricke during a short stay I made in that city. He, as well as his able associates, assured me, that from 1827 to the period I am alluding to, the non-mercurial method of cure had been invariably followed, as they considered its superior efficacy and utility to be perfectly established. I saw the patients under the treatment; I was permitted to examine the day-books and journals; and I, therefore, hesitate not to vouch that the plan, hereafter to be described, was strictly adhered to. About sixty female patients, who had been restored to perfect health, were dismissed one day from the hospital when I was there. They were examined, in a state of perfect nakedness, from head to foot, before they were discharged; and so minute was the examination, that if there had been the slightest excoriation or the smallest wart on any part of the body, it could not have escaped detection. What surprised me as much as their cure, (for at that time I did not believe it to be possible without mercury,) was the healthy appearance of their colour, and the absence of every sign of weakness or exhaustion.

The history of the cases in which mercury was employed before the non-mercurial method was fully established, I pass over entirely. In giving a full and detailed account of the method, I feel as if I ought to be excused on account of its importance, for it will surely not be pretended that the opinions of the very best and

most experienced of British surgeons are unanimous on the comparative efficacy of the two methods of treatment.

If it be assumed that a physician is not a proper judge of disputed points of surgical practice, I beg to observe that I was regularly educated to surgery as well as to medicine; that the situation I held for so many years in Russia made me the protector of a numerous class of surgeons as well as of physicians; and, lastly, that I do not erect myself to be a judge of the two methods, but at present confine myself to the humble task of a narrator and translator of facts.

If I have any doubts of the non-mercurial method of treatment becoming general, especially in private practice, as it is called, the doubt is not founded on the slightest suspicion of the want of efficacy of this method; but it arises from a long and intimate knowledge of the manner of life, habits, and morals of patients who expose themselves to the disease. I believe that three-fourths of them would much rather be treated by mercury, and be allowed to eat, drink, and amuse themselves, than submit to the abstinence and other restraints which the non-mercurial method requires as a *sine quâ non* condition of cure.

I now allow Dr. Gunther to speak for himself, and his friend Dr. Fricke. The WHOLE of what follows is a translation from the Annals; but I request it to be understood that I have abridged the number of cases.

Of the patients who were treated without mercury.

When Dr. Fricke introduced this method into our hospital, he at first selected a few cases only for trial, and he rejected those which there was reason to think would be quickly cured. But as experience soon taught us, (con-

trary to expectation,) that the disease yielded more readily to the non-mercurial than to mercurial treatment, and that the secondary symptoms were not only less frequent, but of a milder character. This method, modified indeed by particular circumstances, was afterwards adopted for all kinds of venereal patients.

At the present moment in which this is written, (February, 1828,) and after a lapse of a year and a half since the non-mercurial plan of cure began, and after one thousand patients have been subjected to it, the result has been so fortunate, that we have no reason for abandoning it or of recurring to the old practice.

The cure of the sick is much speedier than formerly, as has been already stated; secondary symptoms are less frequent, and the patients when dismissed have a much healthier appearance. The unpleasant consequences of salivation are avoided. Formerly it was impossible, notwithstanding the greatest attention to cleanliness, to prevent the bad smell and the foul state of the beds and floors of the venereal wards. The breath of the patients labouring under salivation, and the exhalation from carious bones, poisoned the air. Bad smells and want of cleanliness were *à l'ordre de jour*, whereas at present in the wards in which there are often from sixty to a hundred venereal patients, the air is as good and the cleanliness as great, as in any of the other divisions of the hospital.

To us the venereal disease seems gradually to be assuming a less complicated and more benign character. At least those patients who either have not employed any, or, at most, only a small quantity of mercury, no longer exhibit the malignant forms of the disease. As the right of visiting the hospital and all the patients, is open to every professional man, the means of conviction are at hand.

In consequence of the rigid police to which prostitutes are subject in Hamburgh, and the attention and experience of the surgeons who have the inspection of them, and more especially as these girls are sent to our hospital whatever be the disease with which they are attacked, we are in complete possession of their medical or pathological life.

Every prostitute, whether residing in the town or in that quarter of the suburbs called the Hamburgerberg, are regularly visited twice a week by two authorised or sworn surgeons. Every one of them must bring with her on such occasions a book in which the state of her health is inscribed. Those who are ill and cannot attend, are immediately carried to the hospital.

It is to be lamented that a similar control over the men is impossible. A great number of them leave Hamburgh. Many when infected a second time apply to other surgeons, and are subjected to the usual mercurial treatment. Consequently the result in such cases is never clear or certain. Many of those who have been cured and remain well, and who at the time of leaving the hospital, came under an engagement to show themselves from time to time, do not keep their promise, and a few who lose patience, beg to be discharged when their cure is almost completed, and when they consider themselves well. In the last half year, however, this has not happened once. But all these circumstances render it difficult to know the true result in every case. However, there still remains a number of male patients whom we continue to watch over, and who for years are under our eyes. The women, on the other hand, are, with very few exceptions, constantly under inspection.

We request, however, all medical men, especially those of Hamburgh, who have venereal patients under their care, and who have been treated by us without mercury

to keep a day-book of their disease, and to send it to us either directly or indirectly.

On the General Treatment.

There are four conditions or rules which we are desirous of being observed: cleanliness, quiet, a rigid diet, and an antiphlogistic regimen.

Cleanliness is of the greatest importance for the obtaining a quick and successful cure; for many are restored to health solely by frequently bathing and washing. And, on the other hand, want of cleanliness is frequently the cause of new symptoms, or, at least, of old ones becoming worse.

On entering the hospital, all venereal patients, except those who are quite clean in their person, are bathed and washed. This is seldom necessary with the women; with the men almost always. The diseased parts are to be frequently washed with tepid water. This regulation requires extraordinary attention, as far as regards male patients. The girls, who from nature have a greater regard for their person, do not require the same.

All the sores and excoriations, warts, condylomata and eruptions, the glans penis, and proputium in cases of gonorrhœa, and all carious bones, must be frequently freed from slime, pus, sweat, and other impurities, by means of washing, injections, and aspersions, &c. Purulent matter must not be allowed to accumulate on any sore; and in cases of the spurious gonorrhœa of the glans penis, the diseased mucus must not be allowed to lodge either between that part and the prepuce, or in the folds of the latter.

A rule of the greatest importance is to prevent durable contact between chancres, excoriations, and condylomata, and sound parts which are covered with a fine mucous

membrane, for instance, in the vagina, or between either the smaller or larger labia, between the prepuce and glans penis, and even between the scrotum and inside of the thigh; for if this be not attended to, the sound parts are apt to become inflamed, excoriated, and even ulcerated, and the cure retarded. We are also very careful in removing all diseased mucus from every fold or corner of the genital organs, especially in leucorrhœa and gonorrhœa, and also every kind of puriform discharge from wounds, for if this be not done, excoriations, superficial ulcers, and warts, are apt to arise.

To accomplish this, we lay charpie or lint imbibed with pure water, goulard lotion, or lime water, &c., in every angle, and change them three or four times a day, and often more frequently.

This regard to cleanliness is of the greatest consequence, even after the cure is completed, for if not attended to, the cicatrix is apt to become inflamed, excoriated, and ulcerated, accidents which we have frequently known to have happened to those who have been newly dismissed. But they all got well on their readmission into the hospital, by a more rigid attention to cleanliness.*

Rest is necessary, especially in the first stage, especially when the disease is of an inflammatory character. Therefore, all the patients were rigidly confined to their beds on their first admission. With the female patients this was found to be necessary to the very termination of their cure. To the men, on the other hand, some exercise was allowed, and was indeed found to be useful, when chancres or the wounds of buboes were almost healed, or when they became callous at that period. The only

* Such cases, when they happen to those who have not taken any mercury, may be mistaken or misrepresented as proofs of the disease not having been cured, but as they get well by cleanliness alone, no such conclusion ought in fairness to be drawn from them.—A. C.

reason which can be assigned why the same indulgence is hurtful to women, seems to be in the difference of dress, and the almost impossibility of preventing the contact of parts, or the friction of their clothes. Pregnant women, however, were allowed some exercise.

The diet of the patients in the beginning of their cure consisted of four ounces of wheaten bread, and about a pint of *soupe maigre*, with some flour mixed in it, three times a day, and at dinner six table-spoonfuls of vegetables were given in addition. These last varied with the season. The patients were not allowed either brandy, wine, or even water. If thirsty, barley-water was given them. As soon as the peculiar character of the sores or of the complaint began to disappear, the diet was changed according to the strength and constitution of the patients. In the progress of recovery, animal food was permitted. But in such subjects as were much reduced in strength, and especially in those who had been weakened by a previous course of mercury, we were obliged to depart from the rule, and to allow them meat even in the beginning. As to the female patients, whose treatment seldom requires more than three or four weeks residence in the hospital, and many of whom indeed are restored to health in a fortnight, and whose desire of food is less than that of men, the prescribed diet was continued until the time of their dismissal. The appearance of these women is that of perfect health, and as the duration of the low diet and the time of their confinement in the hospital is not long, they do not lose their strength.

The *medical treatment* is in general not complicated, and of late has been much simplified. In the commencement every patient, whether male or female, was blooded to the amount of from six to twelve ounces, but experience soon taught us that this practice was unnecessary, and that in adhering to the other rules the cure might be

accomplished with equal success and in as short a time as when blood was withdrawn. Blood-letting, therefore, is renounced except in particular cases, where there is extensive inflammation present, or where the patient is of a very full habit. Indeed, it is now seldom employed.

In some cases leeches are applied.

The following prescription is that which is generally given in the beginning of the cure.

℞. Sal. Anglican (Magnesiæ Sulphatis) ℥iss.
Aquæ fœnicul. ℥viij. M.

This is called the *English Mixture.*

Of this solution one tablespoonful is given thrice a day, or sometimes oftener, so that at first the patient is gently purged every day, but afterwards only once a day. In some cases, in which the Epsom salts, when long continued, produces bilious symptoms, an emetic is occasionally given.

In the case of pregnant women, the mixture is given merely with the view of keeping the bowels open, for fear of disposing to abortion. Where secondary symptoms appear, the compound decoction of sarsaparilla and the nitrous acid are exhibited.

After a long-continued use of the *English mixture*, an appearance of aphthæ frequently takes place on the inside of the under lip, or of the cheeks, in the form of little ulcers of from three to four lines in diameter. They have a kind of lustre like satin, are painful, and their edges are often swollen and turned back; they sometimes resemble follicles of the mucous membrane which are filled with a sebaceous substance. Scorbutic and scrofulous patients are most subject to this accident.

The leaving off the use of the Epsom salts, the gargling the mouth frequently with mild astringent or weakly acidulated lotions, or such as contain a small quantity of *tannin*, and, if these fail, the application of a

solution of lunar caustic, heal these sores, which are often of an obstinate nature.

In many cases, salivation has occurred after a full and continued use of the nitric acid; a long-continued use of the decoction of the woods generally produced a trifling increase of perspiration, now and then an increase of saliva. The nitric acid was given according to the following formula:—

℞. Acidi Nitrici, ʒß.
Syrup Sacchari, ℥j.
Decoct. Avenæ, ℥xij. M.

To be marked *Nitric Acid Mixture.*

A tablespoonful was given every two hours, and in many cases every hour. Of the decoction of the woods from eight to twelve ounces were consumed daily.

The baths which were employed were a soap-bath for the sake of cleanliness, and also for some kind of eruptions; and, for other kinds of Exanthemata, baths with common salt, and sometimes with mineral acids, or corrosive sublimate. Baths with caustic potash were chiefly employed in the cases of nocturnal pains of the bones.

Treatment of Particular Symptoms of Syphilis without Mercury.

Gonorrhœa of the glans penis, which is commonly associated with an inflamed constriction of the prepuce, at times as a cause, at others as an effect of the discharge, yielded soon after we succeeded in exposing the whole glans, and washing it, as well as the inner surface of the prepuce, frequently, with a solution of the subacetate of lead. When it was impossible to draw back the prepuce on account of phymosis, a solution of subacetate of lead, and tepid water, were alternately

injected every hour between the prepuce and glans penis.

We have scarcely ever met with a bad case of this kind of gonorrhœa in which there was not some ulceration of the glans. Where it had been neglected, and the patient had led a dissipated and irregular life, and the flow of the puriform discharge was obstructed by the swelling of the prepuce, we generally found extensive and deep ulceration around the crown of the glans. More of this afterwards in the chapter on Phymosis.

Gonorrhœa of the Glans Clitoris.

This is a frequent occurrence. But in the tables the cases are blended with those of Fluor Albus, not to multiply uselessly the number of diseases; and in truth this disease is scarcely ever found alone, but was almost always accompanied by fluor albus and excoriations. Between the clitoris and its prepuce a thick whitish mucus collects, and also in the folds which the clitoris makes with the prepuce, a cheesy kind of matter is also found. The clitoris itself we often discovered to be highly inflamed, ulcerated, and extremely painful when touched.

It generally yielded in a few days to the frequent washing with tepid water or Goulard's lotion.

Blenorrhœa. (Gleet.)

Patients labouring under gleet seldom come to us for advice until they have tried in vain everything which private practitioners can do for them. We have been seldom successful in such cases, and, indeed, have rarely cured those who apply at an earlier period. Few, however, of such patients leave the hospital until the dis-

charge ceases and appears to be cured, but in the greater number of such whom we afterwards met with, the disease had returned.

Of all the remedies we have tried the cubeb pepper has been the most effectual. We gave a teaspoonful of the powder six times a day. In a short time it subdued the discharge so much that a few drops only appeared in the morning and evening. When the medicine ceased to produce the desired effects, we suddenly increased the dose, giving two ounces of it in the day, or else three teaspoonfuls were exhibited thrice a day.

The following mixture did much good in many cases:

℞. Vitell Ovor. No. ij.
Aceti Vini.
Ol. Olivar āā ℥iij.
Balsam. Peruvian, ʒvj. M.

One tablespoonful every two hours.

On many occasions we gave the cubebs alternately with this oleaginous mixture.

In some cases a solution of twelve grains of white vitriol in as many ounces of water injected regularly every hour stopped the discharge. But, on the other hand, we have often given a whole series of approved remedies without much benefit.

In the cases in which pain after making water, and a degree of redness and intumescence of the mouth of the urethra, induced us to try an antiphlogistic treatment, our success was never durable.

Of the Inflammatory Stage of Gonorrhœa.

Our remedies were oleaginous emulsions and the following mixture.

℞. Tinct. Kalin,* ʒij.
Aq. Lauro Ceras, ℥ij.

* What the tinct. kalin is, I do not know, and forgot to inquire.

Dose, thirty-five to fifty drops in a cupful of water-gruel every two hours.

This last mixture, which Dr. Fricke has been in the practice of giving for a great number of years, and which alone frequently cures the complaint, and at all events removes the inflammatory symptoms, was exhibited during eight or ten days successively. The cubebs are then given in the doses already stated; but at first they are combined with nitre, afterwards quite alone. In two cases, in which the inflammatory symptoms were at their greatest height, two teaspoonfuls of the powder of cubebs were given every two hours with the best effect. In both of these cases the pain and discharge were soon diminished, and in a few days the gonorrhœa was cured. But it is to be observed that both of these patients were ordered to keep in bed till the end of the cure.

General syphilis as a consequence of mere gonorrhœa we have never seen.

Gonorrhœa of the Urethra in Females.

We have met with this in a few cases combined with leucorrhœa of the vagina. A burning heat accompanied the flow of urine. A white discharge flowed from the urethra. Frequently washing the extremity of the urethra with Goulard's lotion, and the keeping a leaden catheter for some days in it, generally put a stop to the complaint.

Strictures of the Urethra.

As these were always the consequences of a previous gonorrhœa, we merely mention them, especially as they are not noticed in our tables. In many instances the prudent use of bougies and catgut afforded the best relief.

We anoint the catgut with zinc ointment or saturnine ointment, which answered better than oil. As to the application of caustic after the method of Ducamp, which we tried in some cases, it certainly destroyed the stricture sooner, but the pain was great and the disease returned in a short time. We mean to treat this subject more fully at a future period.

Leucorrhœa in Females.

This disease appeared in two different shapes.

The first consisted in a discharge of a thin fluid like milk. It looked like the water which is at first formed on slaked lime. The discharge was not copious, and it was frequently to be discovered only in the folds of the vagina. Where cleanliness and rest were both neglected the discharge often adhered to the hair of the mons veneris and greater labia, producing erysipelatous inflammation and excoriations. The whole internal surfaces of the inner labia were also frequently ulcerated by this discharge. On the linen this discharge, when dry, left a hard greenish stain, which had a shining look. It is of more frequent occurrence than the following discharge, and is also more easily healed.

This second kind is a rare occurrence with us. The discharged fluid has a greenish-gray aspect, something like thick water-gruel, but much greener. In flowing out between the greater labia it adhered to the hair, which became matted with it. On separating the labia, it was drawn into filaments, in consistence like the thick mucus of the nose, or a thick decoction of salep. It collected itself chiefly behind the place where the hymen previously existed, and might be squeezed out by pressure on the perinæum. Its stain on the linen was much greener

than that of the first, and it seemed to adhere more strongly to the linen.

We treated leucorrhœa in general by means of external remedies alone. Injections of a decoction of elm bark were employed for that kind which was first described, when slight. In worse cases a solution of the sulphate of zinc in the proportion of one scruple of the salt to twelve ounces of water was employed. The second kind was treated by means of the same solution, or else by one of corrosive sublimate in the same proportions.

Where leucorrhœa was complicated with chancres, we made use of the same solutions much diluted, or we laid them aside altogether, and confined ourselves to the cure of the chancres: the leucorrhœa generally disappeared during the treatment. We have not met with a single case of general syphilis arising from this source.

Swelling of the Testicle and Epididymus.

This accident was in general the consequence of neglected gonorrhœa, or of astringent injections injudiciously employed. It was sometimes accompanied by inflammatory fever, in which case the scrotum was highly inflamed, the tumor was as large as a goose's egg, acutely painful when touched, and it was impossible to distinguish by examination the testicle from the epididymus. As soon as the inflammation began to yield, it was then easy to distinguish that its true seat was in the epididymus, and that the testicle itself was comparatively but slightly affected.

The spermatic chord was always more or less affected, and in severe cases it was also painful, hard, and swelled. In some cases the swelling of the chord was so great that

the case might have been mistaken for sarcocele, if its origin had not been known.

The hardness of this tumor was often in an opposite relation to the pain. We never met with a case in which both the testicles were swollen at the same time; but we have seen the disease translated from one to the other.

The swelling of the testicle generally began to yield about the second day after we commenced the treatment which is about to be described. It began to be softened about the fourth, but the epididymus still continued to be very hard. About the fifth or sixth day the spermatic chord commonly began to grow less painful and hard in the superior portion; but its progress was not uniform throughout its length. Hard knots were common about the middle, which yielded very slowly. The swelling of the epididymus gradually grew softer and smaller, and commonly ceased to be painful about the fourteenth day. In a few cases only did the epididymus remain hard and painful for a long time; but it seldom was restored to its natural state and size in less than four or five weeks, and in some cases the *cauda* retained a degree of hardness which never disappeared, or at least it did not do so until after a long lapse of time.

The treatment was very simple. Where there was much fever, bloodletting was employed, and leeches were also applied to the scrotum, and the bleeding from their wounds encouraged as much as possible. Warm applications, sometimes with narcotic plants, were then employed. Internally nitrate of potash, and, when the pain was very great, Dover's powder, (Pulv. Ipecacuanhæ Comp.) Gentle aperients and glysters were employed to keep the bowels open.

When the first stage of inflammation was subdued an

emetic was often requisite; it had great influence on the pain.

The swelling of the epididymus, when not accompanied by fever, as was generally the case, was treated by means of warm cataplasms, leeches, and emetics. But in many instances the pain and hardness did not yield to these remedies. In such cases the fomenting the scrotum for about a quarter of an hour at a time, with equal parts of brandy and vinegar, did much good.

When gonorrhœa accompanied the swelled testicle, we paid no attention to it until the swelling disappeared.

Excoriation of the Genitals.

In appearance this agrees so far with chancre that there is a greater or less loss of epidermis, but what chiefly distinguishes them is that the edges of an excoriated part are not sharp or much elevated above the ulcerated basis. It is easily cured, comparatively with chancre, and is much more common in females than in males.

We have remarked three varieties of excoriation.

The first consisted of numerous fissures in the epidermis, from which a serous fluid issued. The epidermis was of a fiery red colour and much inflamed all around the fissured spot. Exercise or motion increased the pain. Its chief seat in women was the external labia, and in men the scrotum, and in both the inside of the upper part of the thigh nearer to the genital organs. It arose almost always from an accumulation of the matter discharged from gonorrhœa, or the purulent matter of sores, combined with sweat, other impurities, and friction. It was therefore generally found in company with gonorrhœa, chancres, and condylomata in dirty people.

The cure consisted in bathing the patients, in frequently washing the parts, and in the application of pieces of linen wetted with water or a saturnine solution.

Anne S——, twenty years old, of a scrofulous and relaxed constitution, an *unlicensed* prostitute, was admitted into the hospital in November, 1825, on account of swelling, redness, and excoriation of the first kind, affecting especially the external labia. She had also a condyloma on the left internal labium. She had also ulcers around her mouth, and especially at its angles. She was blooded, and was ordered to take a solution of Epsom salts. Diluted saturnine solution was ordered as a wash.

The inflammation of the parts and excoriation began to subside about the eighth day. On the fourteenth the swelling was almost gone. At the end of six weeks, a few very small ulcerated spots remained, and she was discharged cured at the end of the ninth.

She was sent back to us six weeks afterwards, when we discovered a similar excoriation on the greater labia, the angles of the mouth and eyes. The labia were much swollen. She was uncommonly dirty in her person.

She was treated as formerly, and received, on account of the ulcerated state of the eyelids, which was associated with blepharitis, an ointment of red precipitate. At the end of nine weeks she was again dismissed cured.

Sometime after this, she was taken into the hospital as one of the washerwomen, and at present, which is twenty-two months since her last reception as a patient, she has remained perfectly well.

In the second kind of excoriation larger portions of the epidermis were ulcerated. The inner side of the lesser labia and the outer edges of the greater labia among the female patients, and the surface of the glans penis among the men, were the seats of the disease. In many

cases the whole surface of the glans penis was excoriated; it had a satin-like lustre. In women it was usually associated with fluor albus; in men, with the gonorrhœa of the glans penis.

The men commonly got well in ten to fourteen days, by drawing back the prepuce and washing the part with lime water or the saturnine solution. Several got well sooner. The women required a longer time for their recovery, but they had no other remedies, and made use of the same kind of injections.

In many cases these excoriations took place behind the glans, and were soon cured when treated according to the method described; but in some patients who had been much neglected before their admission, the ulceration took on the appearance of real chancres.

Augustus C——, a common labourer, thirty years old, of a robust constitution, was admitted the 1st February, 1827. Fourteen days previously to his admission he had been infected, but he declared he had never before had any symptoms. He laboured under an open bubo, gonorrhœa of the glans penis, a true gonorrhœa from the urethra, ulcers around the frenum, a phimosis, and excoriation of the second kind.

We were only able to draw back the prepuce sufficiently to see that portion of the glans nearest the opening of the urethra, which appeared to be in a sound state. From the contracted mouth of the prepuce a thick greenish white mucus flowed in drops. Pressure on the crown of the glans occasioned pain.

Eight ounces of blood were taken from the patient, and he was restricted to the usual diet. The bubo was laid completely open by means of a crucial incision. Between the prepuce and glans penis tepid water, and a saturnine lotion, were alternately injected every hour; and a linen rag dipped in the Goulard lotion was wrapped around

the prepuce. Daily trials were made to draw back the prepuce, which did not succeed till the eighth day. It then appeared that there were chancres near the frenum, and that the corona of the glans and the internal surface of the prepuce were affected with this second kind of excoriation. These parts were well washed with tepid water, the prepuce was then drawn forward and the injections continued. In four weeks the excoriation and chancres, and in eight weeks the bubo, were all healed. A few days after, the patient left the hospital. Two months after, we had an opportunity of examining the patient, who continued perfectly well; and a year afterwards he assured us of the same thing.

Juliana M——, a prostitute twenty years old, of a sound constitution, infected for the first time, was admitted into the hospital in March, 1827, on account of an extensive excoriation of the second kind, which affected the left internal labium; she had also excoriation about the anus, and a copious leucorrhœa. A Goulard cataplasm was applied to the external parts, and for the fluor albus she employed the remedies already mentioned. She was dismissed perfectly cured the twenty-second day after admission. Since then she has been thrice in the hospital on account of fresh infections, and was dismissed, for the last time, in February, 1828. Until now there has not been a trace of secondary symptoms.

The third variety of excoriation consists in small round superficial ulcers of the epidermis, having a defined circumference without sharp or elevated edges. They resemble the chancres of the first class, and indeed they pass, when neglected, and now and then during the treatment, into real chancres.

Women are more subject to this variety of excoriation than men. It is found in every part of the genitals, but most frequently on the outer and inner labia. In men

it occurs both on the glans penis and the inner surface of the prepuce. These small ulcers are healed more readily than chancres. They are treated exactly like the last described excoriation.

Juliana K——, a prostitute of seven years standing, who declared she had never before been infected, was admitted into the hospital in March, 1827, with excoriation of the great labium of the right side. The ulcerated spot had a diameter of five or six lines; and on the left labium there was another as large as a common pea. On that part of the left thigh which came in contact with the ulcer there was another of the same size. All these ulcers had the appearance already described as characteristic of the third variety of excoriation.

This patient, who was treated in the usual manner, left the hospital quite well twenty-six days after her admission. She was sent back to us ten months after with recent chancres and leucorrhœa, but she was entirely free from any secondary symptoms of syphilis.

Sophia H——, a prostitute of one year's standing, whose skin was of the most delicate texture, and who declared she had never been infected before, was admitted in July, 1826, on account of excoriation of the third kind, affecting the inner surface of the left inner labium and several condylomata. She was treated after the usual manner already described, and was dismissed cured two months after her admission. Five months afterwards she was again admitted into the hospital on account of condylomata, and was cured in fifteen days. Four months still later she came again having excoriation of the greater labium of the right side, and an ulcer of about six lines in diameter at the entrance of the vagina. She was again dismissed five weeks after. Lastly, in December, 1827, consequently seventeen months after her first reception, she presented herself again, on account

of a condylomata at the anus. She remained twelve days with us without any other symptom of syphilis.

Another girl of the town, Wilhelmina H——, twenty years old, whose skin was of delicate texture, and who in the year 1824, on account of chancres and condylomata, had been nine months in the hospital, and again in 1825, had been five months and a half in the same, on account of a bubo, chancres, and condylomata, and had undergone each of these times a mercurial course, was admitted in June 1826, having three excoriated places of the third variety described. One of these affected the left and two the right labium ; besides which, she had fluor albus. She was dismissed cured fourteen days after her admission.

In August, 1826, she came again, having two small chancres on the right labium, and an excoriation of the right tonsil. She remained eighteen days.

In December, 1826, she was again admitted, having two small excoriations of the third kind on the right labium, and two small ulcerated spots in the mucous membrane of the under lip of her mouth. This last evidently arose from the two frequent use of the Epsom salts. Her tonsils were quite sound. She was cured in fifteen days. In August, 1827, she again came under our care on account of a catarrhal inflammation of the eyes; and in October, 1827, on account of two recent chancres, one of which was seated on the left greater labium, and the other on the smaller labium of the same side. That part of the right labium which came in contact with the chancre of the left one was slightly ulcerated. This patient, who had also a discharge from the vagina, was dismissed cured in eighteen days. She made her appearance again in January, 1828, when we discovered an excoriated place, three lines in diameter, between the greater and smaller labium of the left side. It had a

copper-coloured appearance, and there was inflammation around it. She had also excoriation of the first variety on both labia. Her throat was perfectly healthy, and she was dismissed with every appearance of confirmed health nineteen months after she had been for the first time treated without mercury.

Chancre of the Genital Organs.

Of chancres which are distinguished from excoriation by their deep basis, and sharp ragged edges, we have observed seven kinds or varieties, which differ from each other, in respect either of their general appearance, their size, their intensity, or the treatment they require.

First kind.—Chancre with a clean, sometimes copper-coloured surface, which is deeper than the edges; the edges ragged, but not elevated above the cuticle. The diameter from one to six lines, forming a kind of passage from the third variety of excoriation.

Second kind.—Chancre with a sebaceous and generally whitish surface, which is deeper than the edges; the edges ragged, but not elevated. The diameter from one line to two inches.

Third kind.—Chancre with a sebaceous-looking, generally hard surface, deeper than the edges, the edges sharp and ragged, more elevated than the epidermis, reverted, of a deep-red colour, and inflamed. The diameter from one to five lines. (The Hunterian chancre.)

Fourth kind. — Chancre with a very hollow base, covered with a dense, adhesive, thick, sebaceous-looking mucus. The surface irregular, being deeper in some points than others. The edges ragged, elevated, reverted, and in some places of a dark blue colour or gangrenous. Inflammation surrounds the sore. The dia-

meter from three or four lines to one or two inches. Disorganisation of the surrounding parts constant. (Carmichael's phagedænic chancre.)

Fifth kind.—The base of the ulcer not much deeper than the epidermis, but much deeper than the edge of the sore. The edge much elevated above the surface of the skin; not sharp or ragged, but rounded off, and stands like a wall around the basis of the sore. The sore itself is not sebaceous, but generally pale red, and does not exhibit granulations in the beginning. This chancre commonly discharges much serous fluid, producing excoriation of the first kind. Its diameter, including the edges, is about four to six lines. It is often covered with a scab, and forms a transition to the half-globular condyloma.

Sixth kind.—The base or surface of the ulcer more elevated than the edges, of a spongy texture, and reddish-blue colour. Edges undefined; the surrounding skin not inflamed.

Seventh kind.—Hæmorrhoidal chancre. Hard hæmorrhoidal tumors ulcerated on the surface; the ulcers whitish, but not sebaceous. The tumor itself is generally flat and compressed, and full of fissures. This kind of chancre yields a copious discharge of fluid, and is acutely painful. (It passes into the quadrangular condyloma.)

The first kind of chancre frequently passes into the second, third, and fourth, in consequence of neglect, intemperance, constant bodily labour, and want of cleanliness. We found females to be seldom attacked by the fourth kind, and only among maidservants, who, although labouring under the complaint for a considerable period of time, had neglected to take medicines. In men this kind of chancre was commonly seated in the groove between the glans penis and prepuce. The transi-

tion from chancres of the first to those of the other kinds, sometimes took place very slowly, at other times very rapidly; artificial ulcers, which we produced at three different times in women by means of corrosive sublimate, and twice in men by means of common caustic, put on the same appearance as those of the third kind.

In regard to the first symptoms of chancre, those of the first four kinds were of three sorts. The well-known small whitish transparent pustule, either on a sound part, or one that had been previously inflamed. The parts immediately surrounding it became inflamed, and the lymph was converted into a purulent fluid; the pustule then broke, and a chancre of the first kind was immediately formed, which commonly, in the space of twenty-four to forty-eight hours, degenerated into one of the second, and when favoured by the causes already mentioned, soon passed into that of the third and fourth kind. This kind of metamorphosis often took place in the male, very seldom in the female patients.

Secondly, chancres of the first kind seemed to spring from the third variety of excoriations, which either remained stationary, or else were gradually transformed into those of the second, third, and fourth kind. This transition generally took place slowly. Sometimes, however, a slight excoriation of the third kind, in consequence of great neglect on the part of the patient, passed into a phagedænic chancre, in the course of three or four days. We have seen several cases of this in both sexes, especially in men who had chancres behind the crown of the glans penis.

In the third place they formed themselves in the mucous follicles which abound on the external surface of the nymphæ, and which are so often distinctly visible in young woman. These mucous follicles became inflamed and suppurated, producing abscesses when the

aperture of the follicles became closed and the matter was pent up. But when the pus had a free issue, (which happened oftener,) either through the natural opening of the follicle or by bursting it, then ulcers of the second kind arose. In this manner fistulous and *chancrous* sores occurred, which easily escape the sight of an inexperienced eye, on account of their smallness. Prostitutes had frequently sores recurring on the same spot where a fistulous ulcerated mucous follicle existed. These were destroyed by caustic. By this means the spot lost the disposition to be attacked by chancres. These fistulous sores had the appearance of small, thin, membranous, dark-red pustules, which were much deeper coloured than the surrounding healthy mucous membrane. In the centre was a minute opening, out of which matter could be squeezed, and the edges of which were reverted. It often happened that these mucous follicles were closed, and appeared to be healed, but after a short while they burst open again.

Chancres of the fifth kind, where we had opportunities to watch their origin, arose from half-globular condylomata, in consequence of excoriation.

We never had an opportunity of observing the origin of the sixth kind.

The hæmorrhoidal chancres arose from the joint effects of friction and the humidity of leucorrhœa in women being in constant contact with the parts.

In regard to the prognosis, we may assert unconditionally that it was always favourable; for of all the different kinds of chancres, we did not find any which spread much either in diameter or depth, after we began to treat them. Even the phagedænic kind, which had often committed much devastation, before the patients were admitted, were also healed; new and sound granulations replacing in a great degree what had been

destroyed. In a single case, where half the glans penis had been eaten away, the last portion naturally was not restored; but such cases are solitary. Every sore healed perfectly. All the cicatrices were firm and sound.

In regard to each individual kind, experience taught us what follows:

1. The chancres of the first kind were in general healed in the shortest time. Those of the second and third were slower, and those of the fourth the slowest. The spongy or sixth kind held a middle station, and the condylomatous, (the fifth kind,) and the hœmorrhoidal, (the seventh kind,) were frequently very obstinate.

2. The Hunterian chancre, which was sometimes so small that they scarcely exceeded a line in diameter, were slow in healing, comparatively with their size.

3. Chancres which occur in the external part of the prepuce, those on the scrotum, also those in the perinæum, were generally slow of healing; whereas those on the glans penis and behind it, and those on the greater labia, were generally healed quickly. Chancres situated on the frenum in men were frequently long in healing.

4. Those which arose from mucous follicles were seldom cured until the sac was destroyed.

5. In general the cure of chancres was easiest in those who had not employed any medicine before their admission. The most difficult of cure were those which occurred in scorbutic, scrofulous, and phthisical people.

6. Chancres having a brown-coloured surface, were commonly tedious.

7. Solitary cases of very obstinate sores occurred at times.

8. Artificial chancres required about the same time to be cured as the Hunterian ones of equal size.

As to the treatment, independently of what has already been said concerning it, we beg to make the following remarks.

In every case where chancres are found in the folds of the genitals, for instance, between the labia and nymphæ, between these last and the carunculæ myrtiformes, &c., the parts must be separated as much as possible by the fingers, and frequently washed. A little lint, wetted with a solution of sugar of lead or with tepid water, is then to be applied at least thrice a day. If the sore does not soon take on a healing appearance, several lotions are to be alternately employed, such as the saturnine one, lime-water, aqua phagedænica nigra, a solution of four grains of sulphate of zinc in twelve ounces of water, a decoction of elm bark, a solution of sugar of lead combined with oxide of zinc. Some ointments were now and then employed, especially where the chancres were small and dry. The ointments preferred were either the zinc ointment, or the following:

℞. Ung. zinci ℥ss.
Balsam peruvian. ʒj.
Argent. nitrat. fusi et pulv. ϑj.

This was called the *black ointment.*

This ointment was of the greatest use in those cases where the chancre was reduced to a very small size, but where the process of cicatrisation seemed to be arrested. In such cases, we allowed the ointment to remain unchanged for two days on the sore; in short, until it was detached either by the pus or by an eschar.

When the newly-formed skin and cicatrix were rough and disposed to crack, threatening a new wound, we then applied a little zinc ointment with the finger on the surface for some days consecutively.

When the surface of the sore was of a copper colour, althea ointment had a better effect than any other application. But in many cases we were under the necessity of trying various ointments before we found out which was best.

A gentle *pencilling* of the edges of the sore with a solution of the lunar caustic, especially in cases where the healing process seemed to be arrested, favoured the diminution and contraction of the ulcer.

The condylomatous ulcers (fifth kind) were treated in the commencement by means of ablution with weak soapsuds, alternating with Goulard's lotion. They generally became drier, in consequence of this treatment, and granulations sprang up in their centre, while the edges began to approach each other and unite. The half-globular tumors also shrank a little, but it seldom happened that they were quite cured by these means alone. Plenk's liniment we then found to be the best remedy. The prescription is as follows:

℞. Mercur. corrosivi sublim.
Camphor. aa gr. xij.
Alum. crud.
Sacchar. saturn. aa ℈j. misce terendo, et adde
Acet. concentrat. ℥ij. solutioni vitro immissæ, adde
Æther sulphur ʒj.

When the condylomata were anointed for some days with the white sediment of this liniment, they became dry, peeled off, and shrunk. If they withstood the action of this, they were then gently daubed with fuming nitrous acid, and sometimes cut off with scissars. But they often returned from neglect of cleanliness.

The spongy kind were dressed with charpie imbibed in the following solution:

℞. Alumin. crud.
Sulphat. cupri aa ℥ss.
Aq. puræ ℥xij. M.

When its action was too strong, the decoction of elm bark was substituted for it. One or other of these was employed until the fungus was reduced, and a thin light-bluish-coloured skin began to shoot from the edges.

Then recourse was had to the ointment already described.

The sores attendant on phymosis will be treated of in the section on that symptom.

The hæmorrhoidal chancres were also treated with solutions of the subacetate of lead, but we were frequently obliged to destroy the tumors.

We have often tried to destroy, by means of caustic, the small pustule from which chancres originate, before it bursts, with a view of preventing the formation of the ulcer. But much larger sores arose from this cause than if the chancre had been left to itself. In some cases we succeeded, if, as soon as they were discovered on the glans penis, we anointed them with the zinc ointment, and drawing the prepuce over them, allowed them to dry.

We frequently made use of warm cataplasms, and found them of the greatest and most unexpected service under the following circumstances.

a. Where the chancre was much inflamed and painful, the surface dry, the edges red and inflamed; a warm poultice was also applied when the red precipitate produced much inflammation.

b. When the edges were hard, elevated, callous, and turned back.

c. When the surface of the sore was hard, and covered with a thick sebaceous-like secretion.

d. Sores frequently went on well till they were of the size of half of a common pea. They then became stationary, and often grew worse, inasmuch as they became hollow under the edges. In these cases we applied charpie wetted with the decoction of elm bark, and bound it on with slips of adhesive plaster, covering the whole with a warm poultice, and generally succeeded by such means.

e. Poultices frequently healed fistulous chancres of the mucous follicles.

f. We found them very useful in cases of hard and callous cicatrices which were subject to fissure.

g. And lastly, we applied them in those cases in which ulceration broke out after a cicatrix had been formed, first covering the sore with zinc ointment, the black ointment, or some of the lotions already mentioned.

In the treatment of the first four kinds of chancre, we observed occasionally that they often passed through the four stages mentioned. For instance, in the case of phagedænic chancre, after the lapse of three or four, sometimes eight or ten days, the edges grew softer and flatter, and did not turn back so much as before. The sebaceous secretion on the surface was thinner, the gangrenous parts fell off, and new fine red granulations sprang up through small openings of the sebaceous-looking surface. The edge of the sore began to subside on one side, seldom all around at once, the sebaceous-looking surface disappeared totally, and the sore gradually healed.

A large, deeply-penetrating phagedænic sore, either with or without phymosis, was seldom cured in four weeks, generally in six or eight weeks, and at times not sooner than in ten. Those of the second and third kind, after the expiration of three or four weeks; while those of the first kind were often healed in a week, though now and then they required a fortnight, and in a very few cases three weeks.

One of the attendants of the sick-wards K, twenty-two years of age, of a robust constitution, had two ulcers of the first kind on the inner fold of the prepuce, and was placed among the syphilitic patients in October. He was dismissed cured in fourteen days, and continued to serve the hospital for a year afterwards. He then took employment as a common labourer, out of the hospital. Two years and two months afterwards he was

again received into the hospital on account of an ulcer in his leg, but he has never had a symptom of secondary syphilis.

C——, a baker, twenty-six years old, of a healthy, robust constitution, who, according to his own account, had never had any venereal symptom except a gonorrhœa, was received in the hospital in July, 1825. Fourteen days previously to his admission he observed four chancres, two on the glans penis, and two on the prepuce, which seemed to us to be of the first kind. Two of these were as large as a common-sized pea, the two others about five or six lines in diameter. Besides which there was a swelling in the left inguinal glands. The bubo was compressed, and the rest of the general treatment was such as we have described. The only external application which was made use of was spring water.

On the twelfth day all the ulcers were much smaller, and less deep, but three new chancrous pustules appeared on the inside of the prepuce, which were transformed the following day into three small Hunterian chancres.

On the fifteenth day the bubo was opened by means of a crucial incision, and a good deal of pus was discharged.

On the eighteenth day all the chancres were healed, except a small one on the prepuce.

The bubo began to take on a healing process about the twenty-fifth day, but a slight excoriation of the third kind took place on the glans, which gradually assumed the character of a real ulcer. A week afterwards it was completely skinned over, but the cicatrix was a little hard and elevated. In ten weeks the bubo was almost entirely healed, there being only a small ulcer of about four lines in diameter remaining. The patient begged to be dismissed, promising to show himself from time to

time at the hospital; we consented. He came back ten days after his dismissal. The bubo was completely healed and covered by a sound and healthy cicatrix, and scarcely a trace of any cicatrix was to be discovered on the penis itself. The young man himself was about to set out on a pedestrian journey to Vienna. Two and twenty months afterwards he presented himself again, after having travelled over the greatest part of Germany. He had the looks of blooming health, and had not a trace of syphilis on any part of him.

The only internal medicine which we gave him while under our care was a few ounces of Epsom salts; externally spring water was applied to the chancres, and a simple ointment to the bubo.

The prostitute Anna G., twenty-six years old, was for the first time introduced to the hospital in April, 1826; she had two chancres of five or six lines in diameter, one of which was seated in the fossa navicularis, the other on the inside of the right nympha. The great labium of the right side was inflamed, and she had leucorrhœa. The sores were dressed with water alone, but she was ordered a solution of a scruple of blue vitriol in ten ounces of water, as an injection, on account of the discharge. In other respects she was treated in the way described. Three weeks after her admission the sores were much improved. Their progress was hastened by being gently daubed with a solution of lunar caustic. In five weeks both the ulcers were cicatrised, and both fluor albus and redness were gone. She was dismissed after she had been seven weeks in the hospital.

Three weeks afterwards she was again admitted into the hospital on account of an excoriation of the third kind of nearly four lines in diameter, which was seated in the fossa navicularis. This was healed in six weeks by means of the aqua phagedænica nigra or black wash.

She came to us again for the third time nine months after her first admission. She had a chancre of the third kind also seated in the fossa navicularis, being about four or six lines in diameter; and on the anterior edge of the vagina, near the urethra, there was a dry condyloma of the size of a pea.

She had pain in her throat, but which arose from a catarrhal affection. She left the hospital quite well on the twenty-seventh day after her admission, in January, 1827.

Johanna H., another girl of the town, twenty years old, whose skin was of a delicate texture, was admitted in August 1826, on account of two sores seated on the inside of the left nympha. They were both of the second kind of chancre; one of them was not larger than the head of a large pin, the other was a little larger. On the twenty-first day after her admission, both the ulcers being cicatrised some days before, an abscess was formed on the left nympha, which was opened by means of a lancet. She was dismissed the hospital quite cured thirty-two days after her admission.

She was sent to us again in September, 1826, being thirteen days after her dismissal, on account of a chancre which occupied a portion of the carunculæ myrtiformes and vagina. It was about six or eight lines in diameter, and had the character of the second class. She had also a pretty copious discharge from the vagina, the acrimony of which had occasioned a miliary and pustulous kind of eruption on the perinæum. There was also a slight excoriation at the anus. She was dismissed cured at the end of two months and six days.

She came to us for the third time in April, 1827. She had then a cancerous-looking sore on her under lip, which was occasioned by the bite of a sailor. She had not any syphilitic symptom. As soon as the

wound was well, (by means of emollient poultices,) the cervical glands began to swell, and it was long before they were quite reduced. In June a kind of miliary eruption appeared on her forehead, which disappeared after a few baths with nitrous acid. She was dismissed again about the middle of the sixth month from the time of admission.

In October, 1827, she came to us again for the fourth time, and had then, between the greater and lesser labia of both sides, a small excoriation of the third kind, about four lines in diameter. She left us fourteen days after her last admission, that is, fourteen months from the time of her being first received in the hospital, and had not a single symptom either of primary or secondary syphilis.

Lisette Julia M——, another girl of the town, twenty-two years old, who had never had a venereal symptom before, was admitted on the 9th of November, 1825. She had a chancre of the second kind in the fossa navicularis; a good deal of excoriation of the second kind, on the internal surface of the left nympha, and a copious discharge of thick greenish mucus from the vagina. The labia and mons veneris were slightly inflamed. She was treated (internally) in our usual way, and the sore was dressed with the aq. phagedænica nigra. The discharge was diminished about the tenth day after her reception; the sore had a clean surface, and the redness of the labia was less. Both the ulcer and excoriation were healed by the end of the fifth week, and she was dismissed cured at the end of the sixth.

This patient was sent to us again on the 13th of September, 1826, on account of four chancres. One of these, about three or four lines in diameter, was of the first class or kind, with a copper-coloured surface. It was seated on the great lip of the left side. Two

others, which were smaller of the same kind, were on the inside of the right nympha, and close to each other; and the fourth, which belonged to the second kind, was at the opening of the urethra. She was dismissed cured on the twenty-ninth day after her admission.

For the third time she came to us again, on the 6th of December, 1826, on account of two chancres of the first kind, which were seated on the inside of the nympha of the right side. They were about two or three lines in diameter. She had also a half-globular condyloma on the inside of the great labium of the left side, which exuded a fluid.

The condyloma was cured by means of Plenk's liniment. The ulcers were dressed with water. Twenty-two days after her reception she returned home well.

On the 22nd of June, 1827, consequently twenty months from the time of her first admission, she was carefully examined, and found to be in perfect health.

A young man, twenty-two years old, of a robust and plethoric constitution, was admitted in June, 1827. He assured us, that, with the exception of a gonorrhœa, he had never before had any venereal complaint. He had been infected six weeks before he came to us, and had not used any medicine.

On examining him, we discovered four chancres on the genitals. Two of these, of the second kind, were on the inside of the prepuce near its junction with the crown of the glans penis. Each of them was about four or five lines in diameter. The third one (of the first kind) about five or six lines in diameter, was on the upper part of the glans penis. And lastly, the fourth one, which was of the second kind, occupied the mouth of the urethra.

The prepuce was considerably contracted, in consequence of inflammation and tumefaction. It was with

difficulty we could draw it back, and in doing so we squeezed out a quantity of thick greenish matter from between the prepuce and glans.

The right tonsils were slightly ulcerated, and of a deep red colour.

After cleaning the chancres with water, a little lint wetted with a saturnine solution was applied to them, and the prepuce drawn forwards. The same lotion was applied around it. The patient was blooded, and a decoction of elm bark prescribed as a gargle. The drawing back of the prepuce, the cleaning the sores, and application of the saturnine lotion, were repeated daily. At the end of fourteen days his diet was increased a little.

This patient laboured under hydrocele, from which he wished to be freed, which was accomplished by means of incision. Four days after the operation, the chancres healed; a hardness of the spermatic cord of the same side on which the hydrocele existed was gradually dispersed by means of cataplasms.

At the end of six weeks, when the patient left his bed, the ulcer in the throat, which had been almost healed, increased a little in size, but soon after, it began again to get less, and was quite well in the course of a few days. A slight excoriation was discovered on the septum nasi, but which also disappeared a few days after. At the end of two months and fifteen days he left the hospital to go home, quite cured of his chancres and hydrocele. His appetite, which had been impaired, was restored in consequence of the rigid diet to which he had been subjected. On the 7th January, 1828, being four months after he had left the hospital, he paid us a visit to return thanks, and to convince us of his perfect health.

N. N——, a marker in a billiard-room, twenty-two years old, of a robust constitution, but having a

rather sickly atrabilious look, was received on the 27th of June, 1827. He had had at three different times chancres and buboes, for which he had been treated by some of the physicians and surgeons of our city, most probably by means of mercury. He had also had a gonorrhœa once. In the harvest of 1826 he had undergone a cure for a swelling of the periosteum of the frontral bone, which was probably of a rheumatic nature. He told us that about eight weeks before his admission into the hospital he had received a fresh infection in London. In the beginning he had had a chancre on the inside of the prepuce, and afterwards another on the glans penis. For these he had taken pills in London. The inside of his cheeks exhibited the cicatrices of mercurial ulceration.

The chancre on the glans penis was of the second kind, about a line and a half deep, and six lines in diameter. It was of a completely circular shape.

He was treated in the manner described. The saturnine solution was applied to the chancre and around the prepuce, but after eight days there was little change in the appearance; on the contrary, the sore had a more relaxed look. The decoction of the elm bark was now applied on a little lint to the chancre, and the prepuce drawn forward; still there were no symptoms of amelioration.

After a lapse of three weeks passed in this way, the edges of the sore were drawn together by means of adhesive plaster, and great attention was daily paid that matter should not corrode the parts underneath. In six days of this treatment the edges of the ulcer did indeed appear to approach nearer to each other, and to be lower, and the surface of the sore was cleaner. We then applied the decoction of elm bark on lint during the day,

keeping it on by means of the prepuce, and during the night applied the saturnine lotion, after having drawn back the prepuce. The patient was allowed a fuller diet, with permission to take some exercise every day. A condyloma which arose behind the glans was cut off. When the chancre had been diminished to only two lines in diameter, and its surface was almost on a level with the epidermis, which circumstances took place about the end of the sixth week, it then became stationary. The decoction of elm bark was applied on a little lint, and a poultice over that. The sore, at the end of the ninth week, was of the size of the head of a common pin; but the patient had not patience to wait in the hospital till he got quite well, and departed.

He returned eight days afterwards. The ulcer through sheer neglect had become much larger.

He was ordered some decoction of the elm bark, with directions for applying it on lint twice a day, and in the course of three weeks got well, notwithstanding his daily occupation as marker at the billiard table.

Johan Jurgen R——, a sailor belonging to Hamburg, twenty-five years old, of a robust constitution, who until now had never had any venereal disease, was attacked by chancre fourteen days before applying to us, and had taken some powders by order of a physician. He had five chancres; one of them was situated about three lines to the right of the mouth of the urethra, a true Hunterian one of the size of a pea, and three lines deep in the glans. There were two on the internal surface of the prepuce, having a mixed character of the second class and of condyloma. They were deep, having subaceous-looking bases, but instead of ragged they had perfectly round borders, and were of the size of peas. The fourth, which was of the first class or kind, occupied the groove

behind the glans, and was going round the penis. The fifth, another true Hunterian, was immediately under the opening of the urethra near the frenum.

The prepuce was somewhat swollen and inflamed. It was daily washed and surrounded with a bit of lint dipped in the decoctum cort. ulmi. The patient was blooded, and in other respects treated according to the plan described.

In fourteen days all the sores were healed with the exception of the one at the opening of the urethra, which could not be protected by the prepuce. To this a litte charpie dipped in the decoction was applied, and a poultice put over it. He was allowed a better diet at the end of the fourteen days after his admission, and received permission to leave his bed. He went home quite well at the end of the third week.

S——, a combmaker, twenty-five years old, was admitted on the 3rd January, 1826. He had three chancres, all of which were of the second class. Two of these were on the prepuce near the frenum, and the third was on the under surface of the prepuce itself. They were all about three or four lines in diameter.

He had a bubo of the size of a pigeon's egg on the left inguinal glands, which were red, inflamed, and painful. It appeared to us that matter was forming in the bubo. He believed that he had received the infection about eight days before Christmas last. The powders he had taken contained calomel, and he was under salivation when admitted.

We compressed the bubo, and kept the ulcers constantly wetted with the saturnine solution.

About a week afterwards, the fluctuation of matter in the bubo was so distinct that we changed the compression for poultices. On the sixteenth day from that of his admission, we opened the bubo by means of a crucial

incision. The chancres were almost healed, and we now applied a solution of white vitriol to them during eight days. The wound of the bubo was at first dressed with the saturnine solution, afterwards with that of white vitriol, and lastly with the black wash.

At the end of six weeks the edges of the bubo began to adhere : the chancres were closed. In the seventh week the black wash was changed during a few days for cataplasms, and which at a later period were employed at the same time with the green lotion already described.

The patient's diet, which had been increased on the eighth day after the opening of the bubo, was now still more augmented. At last, in consequence of a frequent use of the solution of lunar caustic, and a moderate degree of pressure, a small fistulous canal was formed, but which entirely disappeared by the continued use of cataplasms.

This patient was dismissed cured, after having been three months and six days in our hospital.

He returned for advice on the 15th June, and told us that some days after his dismissal a fistulous sore was formed, which he had quite neglected, and which prevented him from working on account of the pain it occasioned.

This fistulous sore was about an inch long, running from the middle of the bubo under the skin. Excepting this, he had no other appearance of disease. We laid open the fistula from one end to the other, filled it with lint, and put a poultice over it. On the twenty-seventh day it was quite healed, and he was dismissed.

In January, 1827, he returned for the third time on account of a recent gonorrhœa, and left us quite well on the 15th February of the same year ; one year, therefore, from the period of his first admission.

J. M——, a mulatto sailor, twenty-nine years old,

belonging to a North American vessel, a man of a robust constitution, who asserted that he had never had any venereal symptom until at present, was received into the hospital on the 1st of October, 1827, on account of a chancre of the first class at the opening of the urethra, and a rheumatic pain in his shoulders. He had received the infection six weeks before in Philadelphia, and soon after was seized with a gonorrhœa, and a small chancre on the part mentioned. Cubebs were given to him on account of the first of these complaints, but he had not taken any medicine for the other, and had continued his duty without interruption.

The chancre was deep in the glans, having almost scooped it out. The cavity was so great, as easily to have contained half a filbert; and yet the surface of the sore was not *greasy*, but quite clean. A bougie was introduced into the urethra, the hollow sore was filled with lint wetted with the decoction of elm bark. He was not blooded. As a complete cicatrisation could not be effected by these means, a cataplasm was applied. The ulcer was healed about the middle of the sixth week, but there remained a hollow place which might have contained half a kidney bean. He was dismissed cured at the end of the seventh week.

Three months afterwards he again applied to us on account of his chronic rheumatism. The hollow in the glans penis appeared to be a little filled up, and he was quite free from every secondary symptom.

Philip H——, a cabinet-maker, twenty-three years old, formerly scrophulous, who asserted he had never been syphilitic before, was admitted on the 5th October, 1827. He received the infection fourteen days before coming to us, and declared that he had not taken any medicines.

He had two ulcers, one of which was on the upper

part of the penis, between the glans and origin of the prepuce; the other on the internal surface of the prepuce. Both of these belonged to the first class, were about four or five lines in diameter, and had a copper-coloured surface.

After having tried several lotions in vain, they were at last dressed with simple althea ointment on a little lint, which was kept in its place by the prepuce. Soon after this the surface of the sores began to change their appearance and looked better; the edges sunk, and the diameter grew less. When they became quite small, recourse was obliged to be had to the decoction of the elm bark and poultices, by which means a perfect cicatrisation took place, and the patient left us quite well at the end of the seventh week from the day of his admission.

John K——, a shoemaker, twenty-two years old, of slender stature, having a weak and sickly appearance, was admitted in July, 1827, on account of an ulcer on the inner surface of the prepuce, and swelling of the inguinal glands.

About a year before, he had had a gonorrhœa, but no other venereal symptom at that time. About seven weeks ago, after having had a suspicious connexion, he perceived an excoriation on that place where the chancre now was, and had entirely neglected it. When nearly well, he had again connexion, after which the chancre became so painful that he was obliged to apply to us. It was about six lines in diameter, and belonged in part to the second class or kind, and to the nature of condyloma. The surface was of a dirty dark brown greasy aspect, and although not deeper than the epidermis, it appeared to be so, as the edges stood up like a round wall, and were not thin or eroded. It was acutely painful.

In the right groin there was one gland, in the left

two, each of the size of a large filbert nut. The skin covering the tumor of the left side was distended, inflamed, and of a purplish red colour.

The patient was treated in our usual way. The saturnine lotion was applied to the sore, and cataplasms to the inguinal glands. Leeches were frequently applied to the inflamed bubo, until all pain and inflammation were removed; and then a liniment, containing equal parts of emetic tartar and ammonia, was applied twice a day.

At the beginning of the third week, when the edges of the chancre were lower, and its surface cleaner, and the glands in the groin free from pain, the former was dressed with the decoction of elm bark, which was kept on by the prepuce. As to the bubo, a cataplasm was applied during the day, and pressure during the night. The eruption which arose from the liniment was kept up by an occasional re-application of it.

On the eighteenth day after the patient's admission into the hospital, a small chancrous pustule appeared on the glans penis, which dried up after it had been dressed some days with the zinc ointment.

After having applied in vain the saturnine lotion, the zinc ointment, and the black ointment, (already described,) to the original chancre, the copper colour at last disappeared under the employment of the ung. altheæ. It then grew gradually red, granulated, and when at last it was gently stimulated by means of the solution of lunar caustic, it skinned over completely with a sound cuticle. The gland of the left side remained a little hard. This patient was dismissed cured at the end of the seventh week.

C. H. A——, a baker, twenty-eight years old, venereal, as he asserted, for the first time, was admitted on the 19th October, 1827, at which period he had been ill

four weeks, and had taken twelve powders (probably mercurial) by orders of a surgeon.

We found the prepuce, which he could formerly draw back, so much contracted, that we could not see any part of the glans. From between this and the prepuce a great quantity of thick green mucus flowed out. Pressure caused pain.

A poultice was applied, and the saturnine lotion injected every hour, and daily trials were made to draw back the prepuce in order to examine the glans. This succeeded on the fourth day, but with pain. Behind the corona was a considerable accumulation of stinking, dirty fluid, with ulceration of the most remote part of the glans, and two chancres of the second kind, one of which was on the inside of the prepuce, and the other on the frenum.

The glans was cleaned, a bit of linen, wetted with the decoct. cort. ulmi, applied, and the prepuce drawn forward. The injections, and the laying bare the glans daily, were continued until the excoriation was healed. After this we desisted from drawing back the prepuce, as it caused blood to flow each time from the chancre.

About the middle of the fourth week, the ulcer on the frenum was healed and had a sound cicatrix, but the other resisted all our efforts. We now applied the zinc ointment dressing daily, keeping it on by the prepuce; by which means the complete cure of this chancre was effected at the end of a week. After having been five weeks in the hospital, the patient returned to his employment quite well.

Two months afterwards he was again admitted into the hospital, on account of a catarrhal fever. There was not a trace of syphilis to be discovered on him.

L——, a bricklayer, twenty years old, of a delicate

skin, was admitted on the 5th October, 1825. He asserted that he had been infected only fourteen days before this date.

We discovered a very deep and large Hunterian chancre behind the crown of the glans penis, and which went almost round it. Besides this, there were two other chancres of the second class or kind, four or five lines in diameter, on the glans itself, near the opening of the urethra. The whole of the prepuce was gangrenous, and was obliged to be cut off the following day.

At first camphorated spirits of wine, with tincture of myrrh, was applied; afterwards the black wash. The internal treatment was that which has been described.

On the twentieth day after his admission, the two ulcers on the glans penis were healed, and the one behind the glans was reduced to the size of half a common pea. On the twenty-fourth day there remained only a small point which was not skinned over. The patient begged to be discharged, which was granted on the condition of his returning as soon as quite well. He kept his promise, and returned about a week after, perfectly recovered.

N. N——, thirty-nine years old, of an atrabilious constitution, and a hypochondriac, was admitted on account of chancres in October, 1827. He told us that he had had a bubo in consequence of infection some years before, but that it got well of its own accord. The only medicines he had taken were some cathartics. Except a gonorrhœa, he said he had never had any other syphilitic symptoms. He dated the infection from which his present complaint arose four weeks back. One chancre was seated between the glans and prepuce; another, which appeared fourteen days ago, was near the mouth of the urethra. He had not taken any medicines. The chancre, which was in the hollow between the crown of the glans penis and the insertion of the prepuce, was

a true Hunterian one, about seven lines long, three broad, and one and a half deep. Besides these, there were two others on the inside of the prepuce, nearly of the size of a pea, perfectly round, and of the second class. The one near the mouth of the urethra, also of the second kind, had corroded a small portion of the mucous membrane of that canal.

The chancre which was seated behind the glans was very painful, especially on drawing back the prepuce. It suppurated copiously, and the matter flowing between the prepuce and glans, fell in drops from the prepuce. Upon making water he suffered acute pain.

He was ordered to keep his bed constantly; he was put on the usual rigid diet; he was blooded, and the solution of Epsom salts prescribed for him. The prepuce was drawn back once a day, and all the sores, especially the one behind the crown of the glans, were most carefully washed with warm water. They were then dressed with pledgets of fine charpie, cut thin, and wetted with a decoction of elm bark, after which the prepuce was drawn forward. A hollow catheter, six inches long, anointed with the unguentum zinci, was introduced into the urethra, in order to produce a free and easy discharge of the urine, and to prevent it from coming in contact with and irritating the ulcers.

The treatment, however, was so far changed after the first eight days, that the patient was allowed a little fuller diet, and was permitted to be a few hours out of bed daily. A piece of catgut was introduced into the urethra, instead of the hollow bougie, merely to prevent the contraction of the canal. By these means all the ulcers were fully cicatrized in the course of sixteen days from his admission.

He remained fourteen days longer in the hospital on account of a slight itching sensation in the urethra, and

a trifling kind of gleet. He then returned to his former occupation, without waiting till this last-mentioned symptom had entirely disappeared.

We had an opportunity of seeing and examining this man three months later, without finding a trace of syphilis on him. The gleet had entirely left him. His health was better than it had been before, inasmuch as he was now free from his hypochondriacal symptoms.

Anthony M——, eighteen years old, a labourer, of a florid complexion, who, according to his own declaration, had never had any venereal symptoms before, observed, about six weeks before his admission into the general hospital, two small sores on the crown of the glans, for which he took some powders which a surgeon had ordered for him.

Five days before he was admitted he could no longer draw back the prepuce. Upon examination, we found it much inflamed, swollen, thickened, and so much elongated as to conceal the glans completely. It was also so contracted as to prevent, more or less, the flow of urine, and, consequently, gave birth to a scalding cutting pain. From the contracted orifice of the prepuce, a thick, purulent-looking fluid constantly flowed out.

He was subjected to the prescribed regimen, and was also blooded. A saturnine lotion was injected under the prepuce, and pledgets dipt in the same put around it. Nitrate of potash was given internally.

Trials were made daily to draw back the prepuce, which was effected on the sixth day. The glans were then perfectly sound, but on the inner surface of the prepuce near to the frenum there were many real Hunterian chancres. They were cleaned and dressed with bits of thin lint, moistened with the saturnine lotion, after which the prepuce was drawn forward. This was repeated several times a day.

Thirteen days after his admission, when the chancres were nearly healed, we discovered three smaller ulcers of the same kind, which had begun as vesicles, and another which had originated in a mucous follicle. All of them, both old and new, were healed in the fifth week. The patient left us at the end of the sixth.

R. N——, a Jew, thirty-six years old, applied to us for advice on the 2nd of December, 1826, on account of syphilis. Three years previously to this he had had warty excrescences, for which he had taken mercurial pills; afterwards he had laboured several times under gonorrhœa.

His present complaint he ascribed to infection of about five weeks standing. Three days after the impure connexion, a chancre appeared, and not long afterwards, a swelling and inflammation of the right inguinal glands. For these he had employed, according to the advice of a physician, external remedies only, namely, a lotion for the chancre, leeches, and a grey ointment for the bubo. His pale and wan countenance betrayed a dissolute manner of life. He had a Hunterian chancre, that was a little larger than a lentile, on the right side of the frenum, and a bubo in the right groin, about the size of a large hazel nut. The skin which covered it was of a deep-purple colour, and a fluctuation was to be felt in it. It was very painful when touched.

Six ounces of blood were taken from him, and leeches and cataplasms were applied to the bubo, and a saturnine lotion to the chancre.

At bed-time the cataplasms were exchanged for the following plaster:

℞. Empl. diach. simpl.
—— Melilot. aa. Fiat emplastrum.

This is the Green Plaster of the Hamburgh hospital.

The chancre was healed on the ninth day. On the

eleventh day the bubo opened of itself, and discharged a quantity of thin brick-coloured pus. Poultices were applied to it till the seventeenth day, and as it was covered by a thin unsound skin, it was laid open to its edges by means of a crucial incision.

The surface of the sore was flaccid, and was covered with a reddish-brown coloured matter. This was touched every day with a solution of lunar caustic, and then dressed with digestive ointment. In the course of three or four days, the surface was clean and healthy, and granulations were springing up. The edges of the flaps began to subside, to adhere to the surface of the sore, and to be covered with an epidermis, so as to diminish the size of the ulcer. This was now dressed with the saturnine lotion, afterwards with the decoction of elm bark and cataplasms, and at last, as the granulations became spongy, with the green lotion and poultices. By these means the patient, after having been two months and ten days in the hospital, was dismissed cured both of syphilis and dyspepsia. He came to see us six months afterwards. He then enjoyed the most perfect health, and had the intention of going to America.

G. S——, a combmaker, twenty years old, had been cured of two chancres, by means of some powders, (probably mercurial,) six weeks before he came to us. But a new infection, which occurred three weeks ago, had caused a new ulcer. For this he had applied to a surgeon, and had received twenty-four powders to be taken internally, and an ointment of red precipitate for the sore, and a box of mercurial ointment for an incipient bubo. These medicines produced salivation. He was received into the hospital the 13th of June, 1826. We found him in a miserable state. His pulse was feverish, he had lost his appetite, his gums were much inflamed, and he laboured under a pretty large, deep, and acutely

painful Hunterian chancre. It was seated on the upper surface, between the glans and root of the prepuce, and had not only sunk deep into the substance of the former, but had undermined the latter. It was dressed with the ointment of red precipitate.

He was not blooded. The ointment was removed, and an incision was made through that portion of the prepuce which covered the hollow part of the sore, in order to prevent further excavation. By this operation the surface of the ulcer was larger than the deeper one, and measured about an inch in length, and nearly as much in breadth.

It was dressed several times a day with the black lotion, and the usual regimen and treatment was observed.

In a short time the ulcer became cleaner, and its edges sunk down. The diet was increased.

The healing process in the sore was supported by the lunar caustic, and the patient was permitted to leave his bed.

At the end of the seventh week, when the chancre was healed, the right tonsil began to swell, and was slightly painful; but these symptoms disappeared after using an astringent gargle for a few days.

At the end of eight weeks, when the patient left us, the sore was quite healed, the place filled up, and his general state of health was excellent. We saw him three months afterwards, and he was then free from every symptom of syphilis.

F. H——, from Bohemia, a grinder of scissars, about four years before he applied to us, had suffered from chancres for which he had employed mercurial frictions. In January, 1826, he was again infected, for which he received some powders and an ointment from a surgeon, which failing to produce the effect he desired, he took

refuge in the hospital, at which time he was under a slight salivation and inflammation of the gums.

The prepuce was inflamed, contracted, swollen, and could not be drawn back, without very great pain, so as to discover the glans. It had three Hunterian chancres at its outer edge, one of which was from three to four lines in diameter. This one was on the under part. Two smaller chancres were on the dorsum penis. A thick greenish mucus oozed out from the opening of the prepuce.

The prepuce was drawn back with great care and gentleness, but in accomplishing this, much pain was produced. We then discovered a number of ulcers, not only behind the crown of the *glans penis*, but also on the prepuce and the glans itself. We here saw the chancres in their varied progress from one kind to another, inasmuch as some were Hunterian, others belonged to the second class, some to the first, and lastly, some which were fungous, were of the sixth kind. They were all of them covered with a dirty thick slime. They were cleaned as quickly as possible, and the prepuce drawn forwards, for fear of paraphymosis. A saturnine lotion was injected between the prepuce and glans. The same lotion was applied externally. The patient was blooded, and the usual treatment adopted. The prepuce was drawn back once a day, which was easily accomplished on the fourth. The injection and application of the lotion were repeated every hour.

After the space of fourteen days the patient's diet was increased ; at the end of four weeks he was allowed some animal food, and before the end of the fifth week he was dismissed cured.

Henrietta H——, a prostitute, twenty years old, was admitted into the hospital in October, 1826, on account of syphilis, having never before had any symptoms of the

disease. There was a chancre of the second kind in the fossa navicularis, besides a great number of smaller ulcers around the orifice of the vagina. Some of these were of the first, some of the second kind. In each groin there was an incipient bubo, accompanied with much pain, besides which she had a pretty copious leucorrhœa.

She was blooded, leeches were applied to the buboes, and they were compressed. A solution of ten grains of copper in ten ounces of water, was injected into the vagina, a pledget dipt in river water applied to the sores, and the usual regimen observed.

When nearly well, the itch appeared, but which was soon cured. She was allowed to go home at the end of the sixth week.

Five weeks afterwards she was again sent to us with an ulcer in the fossa navicularis, which was nearly five or six lines in diameter. She was again blooded. Half a drachm of the oxide of zinc was added to eight ounces of the solution of the subacetate of lead, and applied to the sore, and in other respects she was healed as usual.

About the seventh day from the time of her admission, when the chancre appeared to be getting clean, small condylomata appeared around its edges. These were touched with Plenk's liniment, which caused them to disappear. She was dismissed at the end of eight weeks.

She returned to the hospital for the third time two months after. She had some pointed nail-like condylomata on the internal surface of the nymphæ, but as she was then under the influence of her catamenia, nothing was done for them, and they disappeared of themselves. The cicatrix of the ulcer in the fossa navicularis was sound and perfectly healed. She was again dismissed on the thirteenth day after her admission into the hospital.

She came to us for the fourth time, two months

afterwards, on account of warty excrescences on the inner surface of the nymphæ. Two days after her admission the catamenia appeared, and when these ceased she was examined and found to be perfectly well. The condylomata had again disappeared without the application of any remedy. The patient was dismissed on the eleventh day after her last admission, being six months from the date of her first admission into the hospital, without a secondary symptom of syphilis. One year after her first applying to us she came again on account of the itch, but in every other respect she was quite well.

Louisa H——, twenty-two years old, of a lymphatic scrofulous habit of body, a prostitute of half a year's standing, infected for the first time, was admitted into the hospital on the 22nd of November, 1826, on account of itch, leucorrhœa, condylomata, and excoriation. The discharge was white like milk, and sparing in quantity; the condylomata were round-shaped, and formed a ridge on the inside of the left nympha. There were also more of them on the inside of the right external labium, and between the left one and the nympha. On this we also discovered an excoriation of the third kind.

She was first treated for the itch, and then our usual means were resorted to for the syphilitic symptoms. A solution of corrosive sublimate was injected into the vagina on account of the leucorrhœa. The condylomata were cut off with scissars. She was dismissed cured at the end of the fifth week from her admission.

Two months after she was again sent to us on account of leucorrhœa and three chancres, one of which was on the inside of the nymphæ, another in the *fossa navicularis*, and a third on the vagina itself. They were all of the first kind.

The discharge was of a white colour and flowed copiously. She was cured in five weeks.

She was again sent to us three months later, with a chancre of the first kind, which was about three lines in diameter, and which was seated in the fossa navicularis. It was healed on the thirteenth day, and she left us on the fifteenth. Two months after she came to us for the fourth time with three chancres, excoriation, many condylomata, and a copious leucorrhœa.

One of the ulcers was seated in the angle between the external labium and the thigh. It was long, and had the character of the third kind. Another was on the left external labium, of the size of a lentil, and belonged to the third kind. The third chancre was on the carunculæ myrtiformes on the left side, and was three lines in diameter.

The condylomata were in groups. One sprung from one of the nymphæ, another arose from the carunculæ myrtiformes of the left side, and two smaller groups had their seat on a small prolapsed part of the vagina. All these were cut off. The leucorrhœa yielded to injections of lime water, and the chancres to the solutions already described. She was dismissed at the end of two months, one year after her first syphilitic symptoms, without having had any other appearances than such as have been described.

The prostitute, Emily H——, twenty-two years old, of a robust make and a scrofulous constitution, who had never been venereal before, was brought to us on the 25th October, 1826, having at the time eight chancres.

One of these, from four to five lines in diameter, was seated on the inside of the left nympha, and another somewhat smaller on the inside of the right one. The character of both of these was of the second class or kind. Three condylomatous ulcers were on the left internal labium, two on the right one, and another of the same description on the upper part of the right thigh. The

condylomatous sores were all healed on the ninth day, and the others at the end of five weeks. She was dismissed at the end of the sixth week.

One year afterwards she was brought to the hospital on account of a violent inflammation of the periosteum of the breast bone, occasioned by her having been run over by the wheel of a cart. As this did not yield to blood-letting, leeches, and other antiphlogistic treatment, an incision was made down to the bone itself, which relieved her from the pain.

Fifteen months after her first admission she returned to the hospital on account of a fresh venereal infection. She had a chancre of the second kind, three Hunterian chancres, and a good deal of excoriation. One of these was seated on the ruins of the hymen, and two others on the outer side of the carunculæ, in the angle between these and the nymphæ. Each of them was about three lines in diameter.

The chancre of the second class was on the inside of the left labium externum.

On both of the labia there were numerous circular excoriations, which were surrounded with elevated edges like condylomatous chancres. They were very red, and exuded much fluid. But others consisted of a kind of pustular elevations, which were not ulcerated.

The patient, who was treated after our usual plan, left us (cured) four weeks after her admission.

Charles Augustus I., a journeyman tailor, robust, somewhat scrofulous, twenty years old, who had not been infected before, was brought to the hospital on the 21st of May, 1827, on account of ulcers. He had caught the disease a fortnight before.

One of the chancres, according to his description, began like a pustule behind the crown of the glans penis. He had applied a little lint to it, but had otherwise totally

neglected it. It spread gradually, occasioning much pain. The pain increased to a great degree in consequence of his being under the necessity of walking much.

The prepuce was much elongated, swollen, of a rose red colour, inflated at the anterior extremity, and funnel shaped. From the opening flowed a thick greenish yellow mucus. We tried to draw the prepuce back, with the view of ascertaining what was the state of the glans. Notwithstanding the acute pain which this occasioned, we succeeded in exposing it as far as the corona; but owing to the hardness and sensibility of some folds of the prepuce which covered the hollow behind the glans, it could not be pulled back.

The whole glans penis was excoriated, red, and of a satin lustre, (excoriation of the second kind.) A Hunterian chancre was close to the orifice of the urethra, and was large and deep enough to have contained a pea. As far as we could discover, the whole of the crown of the glans and the hollow behind it, were eroded by a deep phagedænic chancre, (the fourth kind.)

All the parts which we could expose were washed and dressed with the following lotion, which was also injected under the prepuce.

℞. Tinct. Opii, ʒj.
Aq. Saturnin. ℥xij. M.

The patient was not permitted to inject this himself until he had been fully taught how to do it properly. He was not blooded, but was subjected to the usual regimen. Some days after we were obliged to throw in the injection, as we found the patient could not be trusted to do this, on account of the pain it produced. By this means much mucus and pus were extricated.

In a short time the prepuce could be so far drawn back without much pain, as to expose the half of the

glans. A little ung. zinci on fine lint was insinuated into the deep phagedænic chancre, in order to facilitate the drawing back the prepuce the next time. When all was cleaned, we proceeded with the injections and lotions.

An emetic was ordered on account of some bilious symptoms.

On the third day after the patient's admission, the new trials to draw back the prepuce completely succeeded, though not without much pain. We now discovered several gangrenous spots on the inside of that portion of the prepuce which is reflected near its insertion, and also on the back of the penis. All the surrounding parts were of a fiery red colour, and covered with a chocolate-brown coloured pus. Behind the glans, and under it, there was a deep cavity with a gangrenous surface, the edges of which were ragged, highly inflamed, a little turned outward, and slightly gangrenous, so that the crown of the glans penis was nearly undermined. The frenum also was beset with chancres of the second kind. All the sores were carefully cleaned; pieces of lint cut thin and dipped in the saturnine lotion were applied to the phagedænic chancre behind the glans, a slip of fine linen, also wetted with the lotion, was put around the glans, and then the prepuce drawn forward. This was drawn back by some of us twice a day, and the ulcers most carefully cleaned from purulent matter, and each time treated as has been just described.

On the fourth day eight ounces of blood were taken from the arm.

On the fifth day, the prepuce was easily drawn back without pain. The excoriations of the glans were almost quite healed; the edges of the phagedænic chancre began to have a rounded appearance, and contiguous to them small insular granulations with a bluish epidermis began

to be formed; all the gangrenous parts had sloughed off. In other respects the patient was well, his appetite was good, and he slept well. When the ulcer was a good deal filled up, and the progress of the healing process became stationary, the saturnine solution was exchanged for the zinc ointment, and the decoction of elm bark. This was continued until a perfect cicatrix was formed. The excavation behind the crown of the glans was so much filled up, as to leave a slight hollow only. The patient was dismissed, perfectly cured, on the twenty-sixth day after his admission.

J. J——, thirty-one years old, asserted that, except a gonorrhœa, he had never before been infected. About fourteen days ago he had got a second gonorrhœa, for which a surgeon had given him some powders. Eight days after he was unable to draw back the prepuce, although he had done so with ease until then. A thick greenish-grey mucus flowed from it. On touching the penis behind the glans, he suffered much pain. We succeeded in drawing back the prepuce so far as to expose the whole of the under surface, and about half of the upper one of the glans. So much was now uncovered as to enable us to see that the greater part of the crown of the glans penis was affected with a phagedænic ulcer of one or two lines deep. The patient was restricted to the usual treatment, but was not blooded. He was ordered to inject the saturnine lotion between the glans and prepuce every hour. After persevering in this way four days, during which daily trials were made to draw back the prepuce, and to clean the ulcers as much as possible, we succeeded in drawing it completely back on the fifth day, and thus exposed the whole of the inner lamella of the prepuce. This disclosed a chancre about three quarters of an inch in length, occupying in part the crown of the glans, and in part the groove behind it. It had all the

characters of a true phagedænic ulcer. The inner surface of the prepuce was in part corroded, in part covered with a satiny kind of excoriation, (excoriation of the second kind,) corresponding in situation and size to the chancre. It was also much thickened.

All the parts were carefully washed; a little fine lint, dipped in the decoctum cort. ulmi, was applied to the chancre, the prepuce drawn forward; and this was repeated every day. The daily collection of pus in the chancre was very considerable; the pain occasioned by drawing back the prepuce increased daily; so much so indeed, that at last we were obliged to desist, and to order the patient to inject the saturnine lotion between the prepuce and glans. The consequence was, that the pain yielded, the discharge became less, the prepuce was more moveable, and could at last be drawn back with ease. A second trial was made with lint dipped in the decoct. ulmi, but the symptoms grew worse, the pain and suppuration increased, and it became again impossible to draw back the prepuce. Poultices were applied around the prepuce and glans without any beneficial effect. The charpie was now entirely abandoned, the prepuce was drawn back once a day to clean the ulcers, and the saturnine lotion was injected every hour. By these means, the ulcers became cleaner, and daily smaller, the suppuration was diminished, and the pain ceased entirely.

The patient was now allowed a better diet, and was also permitted to leave his bed and to enjoy the open air. He was dismissed cured at the end of a month.

J. M——, a servant, an Irishman, nineteen years old, who was syphilitic for the first time, said he had got the disease in Ireland, eight weeks before he applied to us. He had sailed soon after to England, where he had got a wash and some mercurial pills, which medicines he had

used in his voyage to Hamburgh. On his arrival, he had recourse to a physician, who prescribed pills for him which soon produced salivation and diarrhœa. He was received by us in the hospital in December, 1827. He was still under the influence of the salivation, complained of great weakness and loss of appetite. On drawing back the prepuce, which was slightly inflamed and œdematous, we discovered a number of Hunterian chancres on the surface of the glans penis, of the circumference of a small pea or lentil; another of the same kind on the prepuce, and two more in the groove between the glans and root of the prepuce. They had corroded a part of the crown of the glans, large enough to contain, with ease, a moderate sized pea.

In this case we again tried lint dipped in the decoctum cort. ulmi, but we were obliged to desist, and to have recourse to injections, for the inflammation, pain, and suppuration were increased. About two weeks after the patient's admission, he was allowed a little fuller diet, and at the end of the fourth week he was permitted to leave his bed. At the end of the ninth week he was dismissed cured.

Some copper-coloured spots, which were higher than the skin, and which had a dull shining aspect, about four or five lines in diameter, disappeared of themselves, without any remedy having been prescribed for them.

S——, a hackney coachman, twenty-four years old, was admitted on the 9th May, 1826, on account of syphilitic ulcers.

Twenty-three months prior to his admission he observed a sore on the penis, and obtained from a barber surgeon a red coloured ointment, a red powder to put into the sore, and some pills and powders to take internally; in consequence of which, he was affected with a violent salivation, and all his teeth became loose. Some

time afterwards, he had a bubo on each groin: these were opened. An abscess, which was seated on the under surface of the penis, had opened of itself. Notwithstanding all this, the patient continued his daily vocation, and at last, as the disease was getting worse, he came to us.

His face was flushed, he had a full and frequent pulse, complained of much pain, and despaired of his recovery. He was a man of a robust constitution. The penis was in a sad plight. Of the skin nothing remained from the point to the symphysis of the pubis, but a little flap which hung from the urethra near the frenum. It was thick, red, and hard. The corpus cavernosum was full of large holes, and the glans was almost detached, hanging only by the urethra. All these parts were much inflamed, very red, and full of pus. Many fistulous sores surrounded the buboes, so as to make the skin like a sieve. On the right side of the scrotum there was a large Hunterian chancre.

The patient was not blooded, but confined to the bed and subjected to the usual treatment. The red precipitate was removed from the ulcer, and cold water applied instead of it.

The day after his admission, his countenance was no longer flushed, the fever was much diminished, and the inflammation and pain were both less. The decoction of the elm bark was now applied externally.

On the fourth, granulations with suppuration began to appear. The patient looked pale and weak. His pulse was of the healthy standard, but feeble. His appetite and stools good. He was ordered a decoction of cinchona.

On the eighth day the urine flowed from an opening in the urethra near the symphysis of the os pubis; at first only in drops, but in the course of three days, more

urine flowed through this opening than by the natural one. The progress of recovery was slow. The flap of skin which hung from the frenum was cut off. From the symphysis of the pubis to the dorsum penis, there were many little flaps of skin, but which did not adhere closely. The ulcers were now dressed with dry lint, he was allowed a fuller diet, consisting of good strong soup, with a dish of light meat; but after five weeks, everything appeared to be in the same state as at the end of the first eight days. The cure had not advanced from that time.

The diet was therefore again diminished, and a saturnine lotion applied morning and evening. A part of the corona of the glans, about three lines in length, and one broad, became blueish black. The fistulous opening in the urethra continued in the same state.

At the end of the first week the surface of the corpus cavernosum penis began to form specks of epidermis, which soon assumed the appearance of streaks and little bridges stretching themselves from the glans to the urethra.

Between the fifth and sixth week, a small piece of epidermis was formed on the right side of the penis. About the same time a violent inflammation took place in the left bubo. The surrounding skin, to a considerable extent, together with the cellular substance, became gangrenous. The appetite was bad, the tongue very red, and Epsom salts were required to keep the belly open.

A decoction of bark, with phosphoric acid, was given internally, and the bubo was dressed with powder of cinchona, camphor, and charcoal powder, and the whole covered with oil of turpentine. Large gangrenous sloughs were detached under great pain. But three days afterwards there were no longer any appearances

of gangrene, the surface of the sore was red, *fresh*, and granulating; but the edges were a little inflamed, elevated, and reverted. The wound was about four inches long and three broad. From this time it was dressed twice a day with decoctum cinchonæ.

Towards the middle of the seventh week the ulcerated groin was very much advanced in the process of healing; the edges of the wound adhered to the surface of the sore, and this was diminished in size by at least one inch. But the granulating process seemed to be arrested, and the surface to be quite uniform. On the penis, however, a new epidermis had been formed, almost all around from the underside of the urethra to the dorsum. But there remained a few spots on this part which were still in the state of ulcer, and which remained stationary. The new epidermis was of a very delicate texture; it cracked in places, but soon healed again. A great part of the glans was skinned over, but this part was only attached to the penis by the urethra as by a stalk. The urine continued to flow through the fistulous opening, but much less than formerly.

We durst not yet venture to introduce a catheter, even with the greatest caution, for fear of tearing off the glans, especially as our patient was very restless.

At the end of three months the state of the patient was as follows. The corpus cavernosum penis was everywhere covered with a good epidermis. The glans was attached only by means of tbe urethra, and some bands of newly-formed skin; it was therefore loose, but quite healed. The fistulous opening was diminished to nearly one line in diameter, and its orifice was cicatrised all around. The patient had begun to wear a hollow elastic bougie; when he made water by its means, none escaped through the fistulous opening. Both the buboes were healed. In other respects the young man was

quite well. He had a fresh and healthy look. He was discharged perfectly cured at the end of the fourth month, the fistulous opening being then closed.

J. B——, a baker, nineteen years old, was admitted into the hospital in August, 1827, on account of syphilis. He asserted, that except a gonorrhœa, he had never had any venereal symptoms before the present infection, which he had contracted four weeks back.

At first he observed a small ulcer on the frenum, for which he had applied to two physicians successively, and from whom he had received a great number of mercurial pills, and also some powders.

The patient, at the time of his applying to us, was under a violent salivation; he was pale and emaciated, and had a number of mercurial sores on the inside of his cheeks. The prepuce was contracted by a phymosis; it was of a rose red colour, thickened, and the apex drawn inward. A thick greenish coloured mucus flowed copiously from its orifice. It was impossible to draw the prepuce so far back as even to have a glimpse of the glans. On the external part of the prepuce, and on the left side, there was a chancre of eight or nine lines in diameter, which was of a round shape. It had eaten completely through the external lamella, and was of the fourth kind. The external surface of the prepuce appeared to be hard, judging by the touch.

On the anterior edge of the prepuce, where the skin is reflected inwards, there were two Hunterian chancres of the size of a pea.

The patient was not blooded. Tepid water and the saturnine lotion were alternately injected every half hour between the prepuce and glans, and then a bit of rag wetted with the saturnine lotion was applied around them. He was confined to the bed and to the rigidly low diet already described, and he was ordered to take a

tablespoonful of the solution of Epsom salts twice a day.

The following day the prepuce was much less inflamed; the discharge from it was much lessened, and we could draw it a little back.

On the fifth day we were able to draw back the prepuce so far as to expose the whole of the glans, but we could not stretch out the thickened folds of the internal lamella, which lay behind the glans covering the groove. We now discovered a chancre of about two lines broad, seated on the frenum, that extended itself to the glans; besides which, there were three others, with elevated and fungous edges. Each of these was about three or four lines in diameter. They were seated on the inner surface of the prepuce, on the tumid part covering the groove behind the glans. From under this swollen part of the prepuce, a thick greenish-coloured fluid was always squeezed out on drawing the prepuce back. It was kept as clean as possible.

On the seventh day, this portion of the prepuce was fully drawn back, and exposed a phagedænic chancre, that was not very large, but which had corroded a part of the glans, and also of the prepuce itself; a pretty extensive excoriation of the second kind surrounded it.

After having cleaned all the ulcerated parts, a bit of thin lint dipped in the saturnine lotion; and five days afterwards, the decoctum cort. ulmi was applied in the same manner to the sores. The prepuce was drawn forward after each dressing.

The chancre of the frenum was nearly well by the end of the fifth week. On the internal surface of the prepuce there was a small point still open, but the chancre on the outside of the prepuce was not larger than a small pea in diameter. All these sores were quite clean, and there was no distinct or perceptible boundary be-

tween the ulcerated part and the epidermis. They did not suppurate more than any insignificant wound of the same dimensions might be expected to do.

The diet of the patient had been a little increased for some days past, and for the last eight days he had been put on full diet. He had not been confined to his bed for the last six days.

After having been one month and twenty-four days in the hospital, the patient was allowed to return home, being quite well. The cure was so perfect, that no loss of substance appeared at any of the cicatrised places.

In the last periods of the treatment we derived the best success from dressings of lint wetted with the decoctum cort. ulmi, over which poultices were applied.

F——, a brewer, twenty-six years old, of a robust constitution, had received infection eight days before, and had taken mercury. He was admitted on the 27th December, 1825. The greater part of the glans was destroyed by a large chancre, the anterior portion only was sound; that is, the opening of the urethra and its circumference; but this portion of the glans was excavated at its posterior edge, and stood up like a loose shield around the glans. All the rest of the glans, to the insertion of the prepuce was wanting. It belonged to Carmichael's phagedænic ulcer, and was partially covered with thick crusts.

The patient was pale, slightly salivated, had highly inflamed gums, was feverish and costive for some days past.

Eight ounces of blood were taken from him. He was ordered the solution of Epsom salts in divided doses every hour till it operated, after which he was to take it thrice a day. Equal parts of the decoctum cinchonæ and water were applied externally.

The following day the inflammation was much diminished, and the crusts fell off.

On the eleventh day the whole of the gangrenous parts were removed, and the surface of the sore looked clean, red, and exhibited healthy granulations.

On the thirteenth day the epidermis began to be formed. The suppuration was moderate; the patient's situation in other respects good. The wound was touched with lunar caustic, and the diet increased.

From the eighteenth day a solution of white vitriol was applied, and the sore was constantly wetted with it. The thin blue epidermis extended itself gradually forward to the middle of the wound; the substance of the glans began to be augmented, and on the thirty-third day from the admission of the patient into the hospital, he was pronounced to be well, but there was a portion of the upper surface of the glans defective.

On the thirty-fifth day the patient left us quite well.

B——, a journeyman baker, of a robust constitution, but of a delicate skin, observed, during a journey, a chancre on the inside of the prepuce, fourteen days before his admission into the hospital. In consequence of his having drawn back the prepuce frequently, a paraphymosis had ensued, and in this state he had walked from Bremen to Hamburgh. On his arrival there, he immediately came to the hospital, and was received on the 18th July, 1826.

The paraphymosis consisted of two highly-inflamed and tumid swellings. Between these there was a gangrenous chancre, running parallel with them, about two lines broad, and an inch long. It had completely eroded the outer lamella of the prepuce, and belonged to the second class or kind. From the frenum hung a large flap, being part of the prepuce.

All attempts to reduce the paraphymosis were fruitless.

An incision was therefore made through both of the tumid folds of the prepuce in the direction of the penis, by which means the stricture was relieved, and the gangrene arrested.

The parts were surrounded with compresses dipped in the following lotion:

℞. Aquæ. Saturnin. ℥xij.
Mucilag. G. Arab. ℥j.
Tinct. Opii. ʒj. M.

He was blooded to the amount of eight ounces, and was ordered to take a table-spoonful of the solution of Epsom salts three times a day, and in other respects was subjected to the prescribed regimen.

The gangrenous parts separated, and the fresh edges began to bend inwards and to adhere, gradually diminishing the size of the ulcer.

The chancre was nearly healed by the fourth week; the patient received a larger allowance of food, and the permission to leave his bed. Nervous pains began to be felt by him in the left arm, in the direction of the *ulnar* nerve, with which he had often been afflicted before. For this he was ordered to rub in the tartar emetic ointment. The chancre was quite healed about the end of the fifth week.

The patient was now removed from the syphilitic to the surgical ward, where he remained with us until January, 1827, therefore fully six months. The pains of his arm yielded after some time to the joint operation of the tartar emetic ointment, the moxa, and aspersions of warm water by means of a *douche.* Soon after, the patient was affected with violent erysipelas in his face, and considerable affection of the brain, which yielded, however, to the common treatment. The patient then left us in good health to return home.

Sophia Maria M——, twenty years old, a prostitute

since three months, who had never before been syphilitic, was admitted in June, 1827. She was covered with filth, especially about the genitals. The whole of the inside of the right internal labium was occupied by a phagedænic chancre of the fourth kind.

At first, compresses, dipped in diluted vinegar, were applied, and as soon as the surface of the ulcer began to look clean, and the sphacelated parts to slough off, recourse was had to the aqua phagedænica and to the saturnine lotion, under which treatment the ulcer got well.

After having been with us one month and fifteen days, she was dismissed cured.

M——, a sugar-baker, thirty-one years old, of a robust constitution, was admitted on the 10th of June, 1826, at which time he had a great number of semi-globular condylomata on the penis, the scrotum, the upper and inner part of the thighs, and around the anus. An acrid ichor oozed out from almost all of them. In some there were minute fissures, forming the excoriation of the first class or kind, but others had a chancrous ulceration in the middle of the condyloma. Several of these condylomatous ulcers were about three or four lines in diameter. The base by which they adhered to the skin might be from six to nine lines. Between most of them there were excoriations.

The patient was blooded to the amount of four ounces only. He was ordered soap baths; to wash the parts affected frequently with tepid water, and afterwards to apply compresses dipped in the saturnine lotion. He was restricted to the usual regimen.

In six days the excoriations, and in fourteen days the chancres, were healed. Some of the excrescences had totally disappeared, while others were shrunk in volume, and had ceased to ooze out any fluid. They still, however,

remained, and seemed to resist the effects of the remedies which were made use of. To these Plenk's solution was applied for some days, and afterwards those situated around the anus, which were the most obstinate, were touched with fuming nitrous acid. This soon caused them to dry, shrivel up, and fall off. After the fourteenth day the patient's diet was gradually increased. In the third week, he received a full portion of food, and the liberty of quitting his bed. At the end of the ninth week he left us quite recovered.

M——, twenty-four years old, a peasant from the nighbourhood of Hamburgh, of a robust constitution, asserted that for the whole of the last year he had not had connexion with any woman, but that having frequently slept in the same bed during the three last months with another male peasant who was syphilitic, he had contracted the disease. He had not taken any remedies. The surface of the penis, the scrotum, the inside of the thighs, and the cavity behind the angle of the jaws, were studded with condylomata, which in part resembled exactly those already described as the fifth kind; while others were funnel-shaped, inasmuch as their basis was smaller and thinner than their upper surface. The centre was ulcerated and pretty deep. The glans penis and prepuce were not in the least affected.

The whole of the scrotum and the inside of the thighs were remarkably painful, inflamed, and beset with excoriation of the first kind. They were covered with mucus, an acrid discharge, and much filth.

Twelve ounces of blood were taken away, and the usual saline purgative mixture and saturnine lotion were prescribed. He was ordered constantly to apply pledgets, wetted with the saturnine lotion, to the various places affected with condylomata; he had several baths, and in

other respects was subjected to our usual regimen. On the back part of the soft palate a Hunterian chancre appeared, of about three or four lines in diameter, for which a gargle of the decoction of elm bark was ordered.

About four days afterwards the discharge of serous fluids ceased, a great deal of the excoriation was healed, but the ulcer was not diminished in size.

On the ninth day the ulcerated parts of the penis were healed, but the excrescences were not yet quite gone. They were distinguished from the skin by their light blue colour. The chancres on the scrotum were less, and had lost in a great degree their funnel shape, inasmuch as their edges were much lower and flatter, and nearly level with the surface of the ulcer. A similar change had taken place on the thighs.

On the tenth day there were no excrescences to be seen, and the places they had occupied were only to be distinguished by the light bluish colour of the part. The ulcer in the throat was healed, but there was still some redness around the cicatrix. The patient left us on the seventh week.

The prostitute, Elizabeth J——, nineteen years old, who had once before been in the hospital, viz. from April to June, 1826, on account of two chancres and cockscomb-like condylomata, was brought to us for the second time in August, on account of some condylomatous chancres of the fifth class, which were seated in the inside of the external labia. At both periods she got well under our usual treatment, and was sent out, cured of the last-mentioned symptoms, the sixteenth day after her admission. We had an opportunity of examining her minutely afterwards, in May, 1827, at which time she was quite well.

J. M——, a manufacturer of cigars, twenty-three years old, of a scrofulous habit, syphilitic for the first time, was

admitted into the hospital on the 14th of December, 1826. Four weeks before he came to the hospital, he had a chancre and bubo, for which he had received mercurial pills and ointment from a physician. He was under a considerable salivation on applying to us.

On the inside of the prepuce he had two fungous chancres, (the sixth kind). The inguinal glands on both sides were hard, but not painful.

He was ordered a gargle on account of the many mercurial ulcers in his mouth, and was healed in other respects in our usual manner, but he was not blooded. The chancre was kept very clean, and the saturnine solution was constantly applied to it. It was healed sooner than the mercurial ulcers, namely, on the thirteenth day after his admission. Eleven days after, he left the hospital quite recovered.

L——, a Swedish sailor, thirty-three years old, of a robust constitution, caught the disorder in Philadelphia, a month, at least, before his admission, and had not taken any medicines for it during the voyage. He had a moderate phymosis and inflammation of the prepuce, from which a thick purulent-looking fluid dropped out.

On drawing back the prepuce, which was done without much difficulty, we discovered a chancre on the upper part of the glans, which was fungous, elevated in its edges, and deep in the centre. It was about nine or ten lines in diameter, and about three in height.

The prepuce was not drawn forward again, and the saturnine lotion was applied for a day or two, and then lunar caustic was employed with freedom. Some days afterwards the green lotion was applied, and when on the nineteenth day it was reduced to a level with the skin, and was not to be distinguished any more from a simple excoriation, it was then dressed with althea ointment, and afterwards with the black ointment (p. 233). The

patient was dismissed cured, after having been one month and three days in the hospital.

Caroline D——, a prostitute, twenty-two years old, having a very delicate skin, and who had formerly been nearly three months (viz. from May to June, 1826) in the hospital on account of a chancre, abscesses, excoriation, and buboes, and who had been cured after our manner, came a second time to us in July of the same year, having at that time a fungous chancre, of which she was also cured.

She came for the third time on account of hæmorrhoidal chancres, (the seventh kind). She had several of them around the anus. They discharged considerably, and caused great pain. Beside these, there was a Hunterian chancre on the left internal labium, and between the buttocks there appeared also a large fungous chancre. By means of great cleanliness, and frequently renewed compresses dipped in the saturnine lotion, which she kept between the buttocks, so as to prevent their pressing on each other, and by means of our common method of treatment, this girl was dismissed cured in the seventh week. Six months afterwards she was accurately examined by us, at which time she had not any symptom of disease.

NOTE.

To these cases many others are added, which are illustrative either of certain varieties of syphilitic affections or of comparative methods of treating the same. How-

ever interesting these may be, and many of them appear to me to be highly so, especially as regards practical surgery, yet I do not deem it necessary to swell this volume by a translation of them, for I must believe that the above forty-two cases are quite sufficient to prove to every candid and competent judge, both the true nature of the disease and the efficacy of the non-mercurial method of cure.

In the fourth volume of the Transactions of the Medical Chirurgical Society of London, there is a very interesting paper on the Venereal Disease in Portugal, by an army-surgeon, W. Ferguson, Esq., the result of whose observations coincide, in one essential point, with those of the physicians and surgeons of the Hamburgh hospital, namely, that both the primary and constitutional symptoms may be perfectly cured without mercury. But his remarks, as well as those of Mr. Rose in the eighth volume of the Transactions of the same society, of Dr. Weatherhead, the physician of the Royal Free Hospital of London, who assures us that he cures syphilis in all its forms without mercury, leaves a doubt in the mind whether the true principle of cure is as yet understood.

Mr. Ferguson and Mr. Rose attribute the cure of syphilis in Portugal without mercury to the action of stimulating diaphoretics of a vegetable nature, such as guaicum wood, sassafras, mezereum, or, in other words, to the Lisbon drink; and it certainly does appear that in the climate of Portugal these were the only remedies made use of by the native physicians and surgeons. It also appears that a very rigid diet was not insisted on. But it is now well known to the medical practitioners of this country that syphilis is not to be cured by the Lisbon diet drink alone.

Dr. Weatherhead, in his History of the Venereal Disease, describes a new mode of curing syphilis without

mercury, which he asserts answers perfectly in this climate.

It consists in the employment of well known diaphoretic and diuretic remedies, such as the oxisulphuret of antimony and sublimed sulphur, together with nitrate of potash.

But there is a very remarkable difference between Dr. Weatherhead's non-mercurial method of treating this disease, and that which has been found to be so eminently successful in the general hospital at Hamburgh.

The Hamburgh physicians and surgeons consider abstinence, bodily quiet, and an uniform temperature, as essential parts of the treatment. With them this opinion is not founded on any speculative reasoning; it is the result of attentive observation and long experience. There never was, I believe, a practical work on any branch of medicine so entirely devoid of theory as the annals of the Hamburgh Hospital; but in strict candour and impartiality it ought to be admitted that Dr. Weatherhead, having an hospital under his care, may with equal justice plead experience as the foundation of his opinion. To reconcile the two opinions is impossible. Dr. Weatherhead not only permits his patients to enjoy a full diet, but he condemns a low one. He says, 'There is 'no more mistaken principle to adopt in the cure of the 'venereal disease than that of keeping the patient low.' I am convinced he has never read the volume of annals I have so often cited, otherwise he would have expressed himself with more reserve. But what I understand as little as his advice concerning the diet of his patients, is his recommending daily exercise in the open air in such a climate as that of England, to patients to whom he gives antimonial diaphoretics every day.

Were it not for the apprehension that I expose myself to the censure of some surgeons who seem to think

that physicians ought not to occupy themselves with the nature or treatment of a disease which in this country is generally consigned to their skill and care, I should be much tempted to make still greater and more numerous extracts from the annals of the Hamburgh hospital relative to syphilis, for I have not as yet adduced any of the many cases of secondary symptoms, or what is called confirmed lues, which also yielded to the non-mercurial method of treatment. They are equally numerous, interesting, and convincing, with those of the primary symptoms, and they prove in a satisfactory manner that in comparing the mercurial with the non-mercurial method of treatment, a greater security against relapses is to be found in the last-mentioned method than in the first. Nevertheless I am convinced, as I have already stated, that the method recommended by Dr. Fricke, Gunther, and their imitators, will not succeed in private practice, because the greater number of patients will not submit to the necessary restriction which this plan imposes, especially if they are encouraged to believe that they may be perfectly cured by means of mercury, or of the oxisulphuret of antimony and sublimed sulphur, and yet be allowed to live as usual.

I think such practice is inconsistent in principle, and in many cases injurious. No doubt young people in general, and especially those of strong constitutions, may resist for a long time the action and effects of a mercurial course, and of exposure to a damp, cold, and variable atmosphere. But that such conduct is dangerous in many other cases I am fully persuaded, from numerous instances which have fallen under my observation. Nodes and carious bones are among its common consequences, and if these be mistaken as evidences of some syphilitic virus still lurking in the system, and the use of

mercury be persisted in, such cases commonly terminate fatally. The destruction of vitality proceeds rapidly under such treatment, nodes and caries multiply, and the patients die from constant pain, irritation, and hectic fever.

Such cases were first attributed about fifty years ago, by Mr. Pearson of the Lock Hospital, and afterwards by many surgeons, to a new poison of a mixed character, which was supposed to be generated by the united action of mercury and cold. But strange to say, this did not lead him to recommend confinement in bed, or in an uniformly warm atmosphere, as an essential part of the treatment, and the consequence was, that his favourite remedies often failed to cure the patients. I write from what I frequently saw at the period I am alluding to.

Many such cases are now successfully cured by iodine, especially in hospitals where the diet and confinement contribute to the recovery of the patient's health.

THE END.

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